THE PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

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"Nec aranearum sane textus ideo melior, quia ex se fila gignunt. Nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

THE PHILOSOPHICAL MAGAZINE.

I. *Description of the Arseniates of Copper and of Iron. By the Count de* BOURNON *.

SECTION I.

Arseniates of Copper.

THE natural combination of the arsenic acid with copper, and the different aspects under which this combination appears, according to the proportions in which these two substances are united, were among those objects of mineralogy respecting which our imperfect knowledge required the aid of study and observation. A new copper mine, lately worked, called Huel Gorland, in the parish of Gwennap, in the county of Cornwall, having, within the last two years, enriched the cabinets of London with some very fine specimens of these arseniates, I have been induced to pay particular attention to them; and I offer the result of my observations to the Royal Society as an acknowledgment of that gratitude which I and all Frenchmen, faithful to their king, ought to feel and profess to a country which has distinguished itself as the protector of honour and loyalty.

Although there appears, according to some German authors, reason to think that arseniate of copper has been found in Silesia, its much greater abundance, as well as the various aspects under which it exists, in the county of Cornwall, may entitle it to be considered as one of the many mineral substances which are peculiar, or nearly so, to England.

Of the various works on mineralogy lately published, there are few which have not mentioned arseniate of copper among the ores of this metal. It seems, however, that some of their authors had no knowledge of this ore, except from the very imperfect account communicated by the celebrated Klaproth, in 1787, in the *Schriften der Gesellschaft Naturforschender Freunde*, vol. vii. in which he has given an interesting sketch

* From *Transactions of the Royal Society of London for* 1801.

of the mineralogy of the county of Cornwall, as far as it was then known. Others ſeem to have poſſeſſed only imperfect ſpecimens of arſeniate of copper, as none of the forms which they attribute to its cryſtals can belong to it. Beſides, they all confound with this ore thoſe cubic cryſtals, of a very beautiful green colour, which are found in Muttrell mine, contiguous to Huel Gorland mine; and which, according to the analyſis, made with the greateſt care and ability, by Mr. Chenevix, are of a nature totally different, and cannot properly be claſſed among copper ores, as they contain but a very inconſiderable quantity of that metal.

The exiſtence of arſeniate of copper ſeems, however, even at this day, to be an object of doubt among the French mineralogiſts; for the abbé Haüy does not mention it in the 28th and following numbers of the *Journal des Mines*, although they contain an intereſting extract of a ſyſtem of mineralogy, which he was then preparing for the preſs; nor has M. Fourcroy even hinted at it, in his *Syſtème des Connoiſſances Chimiques*, lately publiſhed.

It is now above twenty years ſince arſeniate of copper was diſcovered in the county of Cornwall; it was firſt found either in Carrach mine, in the pariſh of Gwennap, or in Tincroft mine, in the pariſh of Allogan. Its matrix, like that of almoſt all the copper ores of this country, was ſiliceous, and conſiſted of a decompoſed granite, of which the greateſt part of the feld-ſpar had paſſed into the ſtate known by the name of *kaolin*. It was accompanied with gray vitreous copper ore, frequently in conſiderable maſſes; alſo with much black oxide of copper; and with various oxides of iron.

The arſeniate here ſpoken of, which had never been found in large quantity, had ceaſed to exiſt in the above-mentioned mines, when Huel Gorland mine, lately wrought, began to enrich mineralogy with this uncommon ſubſtance. The matrix of this is likewiſe ſiliceous; ſometimes cryſtalline; and ſometimes in an amorphous maſs. Here and there we find mixed with it, in greater or leſs profuſion, all the known oxides of copper; many of the argillaceous oxides of iron; alſo gray vitreous copper ore; arſenical pyrites; and the rich deep-coloured yellow copper ore. This laſt is often found differing from its uſual appearance, in a manner which, I believe, has not hitherto been taken notice of. I think it ſhould make a diſtinct variety among the deep yellow copper ores, under the name of *yellow hematitic copper ore*.

When the combination of copper with iron and ſulphur is rich in metal, (for, when it is poor, it is only a martial pyrites mixed with a little copper,) its appropriate colour, when

when a piece is freſh broken, is a deep yellow; and this yellow colour is more deep in proportion as the quantity of copper is more abundant. In its richeſt ſtate, it aſſumes a more or leſs greeniſh tint. The ſurface of a freſh fracture is very brilliant, and appears rather uneven, as if compoſed of ſmall laminæ croſſing one another in an irregular manner. When it begins to decompoſe, its ſurface is covered with the moſt beautiful colours; among which the moſt predominant are, violet, blue, and green: this has cauſed it to be compared to a pigeon's breaſt. When theſe colours are very deep, and occupy the whole ſurface of a piece, we commonly perceive, here and there, ſome little points in the ſtate of red oxide of iron; and others of a green colour, in the ſtate of green carbonate of copper.

This kind of copper ore is ſometimes of a deep yellow colour, which inclines the more to green, as it is deſtitute of brilliancy. It is very compact, and, when broken, the fracture appears ſmooth, ſometimes a little conchoidal; its ſurface, however, has a very fine grain, which, when viewed with a powerful lens, reſembles the aggregation of a very cloſe compact maſs of the fineſt ſand. Its moſt uſual texture is that of thin layers or plates, lying one upon another, and being very cloſely united, ſo as to be ſcarcely perceptible to the naked eye; but they are very readily diſtinguiſhed with the lens. Theſe layers, however, do not adhere very ſtrongly, as they may always be ſeparated by the ſtroke of a hammer.

This ore frequently aſſumes a mamillated form; the mamillæ being of different dimenſions, from the ſize of a man's head, and even larger, to that of a ſmall pea. In the latter caſe, the mamillæ are very often united, as in that kind of iron ore which is called cluſter, botryoid, or kidney hematites. Sometimes the ſurface of the mamillæ is covered with ſmall points; but more frequently it is ſmooth, ſo as very much to reſemble a piece of poliſhed metal; and, as the ſurface of the mamillæ inclines rather to a brown colour, they have the appearance of antique bronze. The green oxide of copper, which ſometimes is obſerved on it, completes the illuſion, by aſſuming the aſpect of that fine patina which often covers pieces of antique bronze.

This ore is likewiſe found in the form of ſmall cylinders, often placed one againſt the other, and ſometimes ramified, in the ſame way as is obſerved in ſome kinds of hematites. When the broken ſurface of it is expoſed for ſome time to the air, it aſſumes the colour of tarniſhed gold. It acquires alſo, by the decompoſition of its ſurface, the ſame violet,

blue, and green colours, as the kind already deſcribed; but, although theſe colours are frequently very deep, they never are ſo brilliant as in that kind.

It is very uncommon to find pieces of this ore that are not mixed, and frequently even penetrated, with gray vitreous copper ore. That which accompanies the arſeniate of Huel Gorland mine, offers, in this reſpect, an uncommon and very particular appearance. The yellow ore is mechanically mixed with the vitreous ore, ſo as to form a compound, in which, by the aſſiſtance of a lens, the ſmall particles belonging to each of thoſe two ores may be very clearly diſtinguiſhed. The ſpecific gravity, as well as the quantity of copper, in this ore, varies conſiderably, according to the proportions in which the yellow copper ore and gray vitreous copper ore are mixed together; ſometimes they ſeem to be mixed in equal proportions, or nearly ſo.

Nature has eſtabliſhed very remarkable differences between the arſeniates of copper; and theſe take place not only in their forms, but likewiſe in their hardneſs and ſpecific gravity. Theſe differences ariſe, either from the manner in which the arſenic acid is combined with the copper, or from the different proportions in which theſe two ſubſtances are combined. I have been naturally led to follow the ſame order, and to divide the arſeniates of copper into four different ſpecies: and the very intereſting analyſis of this ſubſtance, made by Mr. Chenevix, has afforded me the moſt ſatisfactory ſanction to this diviſion. It is thus that the chemiſt and the naturaliſt, by freely uniting their labours, without jealouſy or prejudice, ought in all caſes to proceed, in order to attain that certainty which is the deſirable recompenſe of their endeavours.

First Species.—*Arſeniate of Copper in the Form of an obtuſe Octaedron.*

The moſt ſimple form under which this ſpecies appears, is a very obtuſe octaedron, formed by the united baſes of two tetraedral pyramids, with iſoſceles triangular planes; and this appears to be its original form. This octaedron has, in each of its pyramids, two oppoſite planes more inclined than the other two; which gives a parallelogrammic form to their common baſe. (Fig. 1. Plate I.) The two planes more inclined than the others, meet at the apex of each of the pyramids, in an angle of 130°; and at the common baſe, in one of 50°. The two planes which are leſs inclined, meet at the apex, in an angle of 115°; and at the baſe, in one of 65°.

Theſe planes are commonly ſmooth and bright; ſometimes, however, they are ſtriated in a direction parallel to their edges.

The four planes ſeldom terminate in one and the ſame point: more commonly the apex is formed into a ridge, the octaedron being lengthened, parallel to its leſs inclined planes; the baſe is then a ſquare, or at leaſt approaches very nearly to that form. (Fig. 2.)

Theſe two varieties are the only ones I have obſerved in the form of the cryſtals of this ſpecies, although I have had the opportunity of examining a great number of ſpecimens.

This arſeniate is very light; its average ſpecific gravity, taken on ſix pieces perfectly pure, was 2,881. Its hardneſs is likewiſe very inconſiderable; it eaſily ſcratches calcareous ſpar, but makes no impreſſion on fluor ſpar.

It is ſeldom perfectly tranſparent, but has generally a cloudy aſpect.

The uſual colour of this ſpecies (for this character is as eſſential in metallic ſubſtances as it is immaterial in ſtones) is a beautiful deep ſky-blue; ſometimes, but very rarely, it inclines more or leſs to Pruſſian blue. It is frequently of a very fine graſs green; the cryſtals have then a much more beautiful tranſparency. I have ſeen ſome which were of a fine apple green; others white, having a ſlight blue caſt. In one piece, the cryſtals of which were of a green colour, and leſs tranſparent than they commonly are, I diſcovered, on breaking them, that the colour of their central part, for about half their thickneſs, was blue. From the obſervations made by Mr. Chenevix in his analyſis of theſe arſeniates, it appears that the variation in their colour principally depends on the quantity of water which enters as a conſtituent part into their formation.

This ſpecies is found mixed with all the other kinds of arſenical copper ore; but that which moſt commonly accompanies it, is the priſmatic triedral ſpecies.

I have never diſcovered in this ſpecies any thing which could induce me to ſuppoſe it ſuſceptible of decompoſition, or even of change.

Second Species.—*Arſeniate of Copper in hexaedral Laminæ, with inclined Sides.*

This ſpecies is commonly found in very fine hexaedral laminæ. The ſix narrow ſides of theſe laminæ have an inclined poſition, alternately in a contrary direction, on the two broad planes, in ſuch a manner that each of the planes is encompaſſed by three ſides, which are inclined upon

it. (Fig. 3.) As far as the ſmall ſize, and more particularly the thinneſs of theſe cryſtals, has enabled me to judge, two of theſe three inclined ſides form an angle of about 135° with the broad planes on which they incline; and the third, one of 115°.

The two broad planes are ſmooth, and have a very brilliant luſtre. The ſix narrow ſides are rendered very dull by the great number of ſtriæ with which they are covered; moſt of which are very prominent, and all are parallel to the edges of the broad planes. In conſequence of this, theſe cryſtals may be divided, parallel to the planes, almoſt as eaſily as cryſtals of mica.

This ſtructure prevents the cryſtal from being conſidered as a modification of the octaedron: that which would be produced by an increaſe of the inclined ſides, would only be a ſecondary cryſtal; and none of the ſpecimens I have ſeen give me reaſon to ſuppoſe the exiſtence of ſuch a variety.

The colour of this ſpecies is a fine deep emerald green; ſometimes, though rarely, it is found of a lighter colour. The luſtre of its broad planes, which are the only parts of the cryſtal that forcibly ſtrike the eye, give it pretty much the appearance of thoſe coloured metal plates which are known by the name of *foil*.

This ſpecies is ſtill leſs heavy than the preceding, its ſpecific gravity being only 2,548.

It is alſo leſs hard; it eaſily ſcratches gypſum, but not calcareous ſpar.

When its cryſtals are very thin, they are very tranſparent; but their tranſparency is diminiſhed when they have any degree of thickneſs.

When expoſed to fire, this ſpecies decrepitates very ſtrongly.

This arſeniate, the matrix of which is generally quartzous, is occaſionally found mixed with ſome other arſeniates of copper, and particularly with the acute octaedron in the capillary or fibrous ſtate. (Spec. 3. Var. 1 and 2.) But the ore moſt commonly found with it is the red copper ore, which is frequently very abundant.

I have never obſerved in this ſpecies any appearance of decompoſition.

Third Species.—*Arſeniate of Copper in the Form of an acute Octaedron.*

The moſt ſimple form in which this third ſpecies is found, is likewiſe an octaedron; but this octaedron, inſtead of being obtuſe, like that of the firſt ſpecies, is ſlightly acute. It has, like that, in each of its pyramids, two oppoſite planes more inclined

inclined than the other two. The more inclined planes meet at the apex in an angle of 84°; and at the baſe, in one of 96°: the other two meet at the apex in an angle of 68°; and at the baſe, in one of 112°. (Fig. 4.)

In this octaedron it ſometimes happens, that the planes which compoſe its pyramids tend to one and the ſame point, in order to form the apex; but it is much more common to find it extended in a line parallel to the leſs inclined planes of the pyramid. (Fig. 5.) The cryſtal is ſtill more frequently found in the form of a long tetraedral rhomboidal priſm of 84° and 96°, terminated by a diedral apex, with iſoſceles triangular planes, which are placed on the angles of 84°, and meet in an angle of of 112°. (Fig. 6.)

Moſt commonly, both in the perfect and the lengthened octaedron, the angles of 96° are replaced by a plane, which is equally inclined on the adjacent ſides, (fig. 7.) and is frequently very broad: (fig. 8.) then the tetraedral priſm of 84° and 96°, is changed into a flat hexaedral priſm, having two angles of 84°, and the other four of 138°. I never ſaw the angles of 84° replaced.

The average ſpecific gravity of this arſeniate of copper, taken on five pure pieces, was 4,280.

It is ſufficiently hard to ſcratch fluor ſpar, but is not hard enough to ſcratch glaſs.

Its uſual colour is a brown, or bottle green, ſo dark that the cryſtals appear of a blackiſh colour, when they are not oppoſed to the light: ſometimes, but very ſeldom, in the regular cryſtals, which happen to be rather thicker, this colour is a clearer green; in other ſpecimens the cryſtals have a yellowiſh caſt, and the ſurface then often reflects the light of a golden tint.

The tranſparency of this ſpecies is generally pretty great.

It is not always cryſtallized in a determinate form, but is an abſolute Proteus, both with reſpect to the different forms in which it appears, and the various colours it exhibits. I have obſerved the five following varieties of it:

Variety 1. *Capillary, of a determinate Form.*

In this variety, the cryſtals are extremely ſlender, yet preſerve their form, which is that of a very lengthened octaedron. The ſmall ſlender cryſtals often form themſelves into a confuſed group; ſometimes, however, they form ſmall mamillæ, by the divergence of a number of them from a common centre. Their colour is either a fine graſs green, or a yellowiſh green, or a golden yellow; and they have generally a beautiful tranſparency.

Variety 2. *Capillary, of an indeterminate Form.*

In this variety, the very thin needle-like crystals are not terminated by the diedral apex of 112°, representing two planes of the octaedron, but gradually become smaller, and terminate in a very sharp pyramid. This variety has the same colours as the preceding; and its very slender crystals are grouped in the same manner as in that.

Substances in a crystallized state, in passing from a determinate form to an indeterminate or fibrous one, frequently assume an intermediate form, in which the crystal insensibly terminates in a very acute pyramid.

Variety 3. *In Crystals perfectly regular for a Part of their Length, and fibrous at their Extremity.*

In this variety, the crystals are perfect during a part of their length; but their substance insensibly divides as it approaches the extremity, which very often is in fact nothing but a cluster of extremely delicate fibres, the colour of which always appears lighter than that of the solid part of the crystal.

Variety 4. *Amianthiform.*

This variety is composed of fibres as delicate as those of amianthus, of the flexibility of which they frequently possess a certain degree. These fibres are either parallel or divergent from one common centre, in which case they nearly resemble a hair pencil. Their colour varies considerably: I have seen them of different shades of green, from a grass green to a dark brown green, of a golden brown, of a straw colour, of a golden yellow, of a greenish blue colour; and even perfectly white, having frequently the lustre of satin.

The fibres are sometimes so delicate, so short, and so confusedly grouped together, that the whole appears like a dusty cottony mass, the true nature of which is discoverable only by the lens. At other times, this variety appears in small thin laminæ, rather flexible; sometimes scarcely perceptible to the naked eye, sometimes tolerably large, and perfectly like *amianthus papyraceus*. I have seen the last-mentioned form of this variety, of a light green colour, and also of a very delicate white.

Variety 5. *Hematitiform.*

This variety is in layers, either flat or mamillated; and is of a fibrous texture; but is rendered compact by the close manner in which the fibres are united to each other, in the same way as is observed in many martial hematites, and more particularly in that kind of tin ore which is known by the

the name of *wood tin*, to which ſome pieces of this arſeniate of copper have a very great reſemblance. Yet it ſometimes happens, as in many aggregate pyrites of a globular form, that the ſurface of the ſmall mamillæ is covered with little rough points: theſe are the diedral apices, which terminate the little cryſtals ſuppoſed to contribute to their formation.

This hematitic variety is found with the ſame diverſity of colours as the preceding, or amianthiform variety.

Fourth Species.—*Arſeniate of Copper in the Form of a triedral Priſm.*

The primitive form of this ſpecies is a triedral priſm, the baſes of which are equilateral triangles, (fig. 9.): this priſm is often conſiderably lengthened in a direction parallel to one of its baſes. (Fig. 10.) This form is one of the moſt rare in cryſtallography. The cryſtals have all their ſides ſmooth and brilliant; yet there are obſervable in ſome of them, when examined with a magnifying glaſs, tranſverſe ſtriæ on the ſides of the priſm, all of which are parallel to the edges of the baſes. It is therefore chiefly on the planes of the baſes that the cryſtalline laminæ appear laid upon one another, to produce either the increaſe or the modification of the primitive cryſtal.

As the cryſtals of this ſpecies are ſeldom ſufficiently detached to be eaſily perceived, and indeed are very frequently ſo ſmall as to eſcape the obſervation of the naked eye, I think it neceſſary to deſcribe here all the various forms in which I have ſeen them, with the progreſs obſerved in their paſſage from one form to the other, however ſmall the difference between theſe forms may appear. Such a deſcription will lead to a better knowledge, not only of the primitive cryſtal itſelf, but alſo of thoſe forms of it which ſeem to be the moſt diſtant from its original one.

Very frequently the triedral priſm paſſes to a tetraedral modification by the ſimple replacing of one of its edges by a plane equally inclined on the adjacent ones. This plane is either very narrow, (fig. 11.) or of a more conſiderable width. (Fig. 12.) Sometimes the width of the plane is ſuch, that it reduces the primitive adjacent planes to extreme narrowneſs. (Fig. 13.) In this laſt caſe, the cryſtal appears under the form of a rectangular plate or lamina, having two of its narrow oppoſite ſides or planes inclined, in one and the ſame direction, on one of the two broad planes. It ſometimes happens, in this variety, that the two broad oppoſite planes approach more or leſs to a ſquare form. (Fig. 14.) I have alſo ſeen ſome cryſtals in which the two other edges of the

priſm

priſm ſeemed to have likewiſe very ſlight ſecondary planes; but, when that happens, they are always very narrow, eſpecially when compared with the ſecondary plane of the third edge. This modification, in its various forms, is the moſt common one of this ſpecies.

Sometimes one of the ſolid angles of the triedral priſm is replaced, on one ſide only, by a plane that is much inclined to the edge of the priſm on which it is ſituated; but the cryſtals are always too ſmall to admit of being meaſured with preciſion. (Fig. 15.) This plane, aſſuming a more conſiderable extent, replaces the ſame edge of the priſm by another plane, much broader at one extremity than the other, as is ſhown by the lines of large dots in the ſame figure. Sometimes it has a very conſiderable extent, as is repreſented in fig. 16. It then reduces one of the baſes of the triedral priſm to the form of a very narrow trapezium, while that of the oppoſite baſe remains very broad. By a ſtill more conſiderable increaſe, the plane of this baſe totally diſappears, and the cryſtal is terminated at that extremity by a ridge. (Fig. 17.) In this variety, the cryſtal is often ſeen placed upon one of its ſcalene triangular ſides, and then preſents the other at its upper extremity; an appearance which is apt to puzzle the obſerver, particularly when he perceives, among the triangular ſides which moſt generally terminate the cryſtals, nothing but equilateral triangles. This modification, in all its forms, is much leſs common than the preceding one; it is, however, occaſionally met with.

[To be continued.]

II. *On preſerving Freſh Water ſweet during long Voyages.* By Samuel Bentham, *Eſq.**

THE Society for the Encouragement of Arts, &c. having thought proper to offer a premium in order to aſcertain, for the uſe of the public, the beſt mode of preſerving freſh water ſweet at ſea, I requeſt you to lay before the Society an account of the method which I have employed for this purpoſe on board two ſhips, and which has been attended with all the ſucceſs that can be reaſonably expected.

The mode in which I conceived freſh water might be preſerved ſweet, was merely by keeping it in veſſels of which the interior lining at leaſt ſhould be of ſuch a ſubſtance as

* From the *Tranſactions of the Society of Arts, &c. Adelphi, London, for* 1801.—The Society awarded their gold medal to Mr. Bentham for this communication.

ſhould

ſhould not be acted upon by the water, ſo as to become a cauſe of contamination. Accordingly, on board the two ſhips here alluded to, the greater part of the water was kept, not in caſks, but in caſes or tanks, which, though they were made of wood, on account of ſtrength, were lined with metallic plates, of the kind manufactured by Mr. Charles Wyatt, of Bridge-ſtreet, under the denomination of tinned copper-ſheets; and the junctures of the plates or ſheets were ſoldered together, ſo that the tightneſs of the caſes depended entirely on the lining, the water having no where acceſs to the wood. The ſhape of theſe caſes was adapted to that of the hold of the ſhip, ſome of them being made to fit cloſe under the platform, by which means the quantity of water ſtowed was conſiderably greater than could have been ſtowed, in the ſame ſpace, by means of caſks; and thereby the ſtowage room on board ſhip was very much increaſed.

The quantity of water kept in this manner on board each ſhip was about forty tons, divided into ſixteen tanks; and there was likewiſe on board each of the ſhips about thirty tons ſtowed in caſks as uſual.

As the ſtowing the water in tanks was conſidered as an experiment, the water in the caſks was uſed in preference; that in the tanks being reſerved for occaſions of neceſſity, excepting that a ſmall quantity of it was uſed occaſionally for the purpoſe of aſcertaining its purity, or when the water in the caſks was deemed, when compared with that in the tanks, too bad for uſe.

The water in thirteen of the tanks on board one ſhip, and in all the tanks on board the other, was always as ſweet as when firſt taken from the ſource; but in the other three of the tanks, on board one ſhip, the water was found to be more or leſs tainted as in the caſks. This difference, however, is eaſily accounted for, by ſuppoſing that the water of theſe tanks was contaminated before it was put into them; for in fact the whole of the water was brought on board in caſks for the purpoſe of filling the tanks, and no particular care was taken to taſte the water at the time of taking it on board.

After the water, kept in this manner, had remained on board a length of time which was deemed ſufficient for experiment, it was uſed out, and the tanks were repleniſhed as occaſion required: but in ſome of the tanks, on board one ſhip at leaſt, the original water had remained three years and a half, as appears by the certificates herewith incloſed. About twenty-five gallons of the water, which had remained this length of time in the ſhip, are ſent to the Society, in two veſ-

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ſels made of the ſame ſort of tinned copper with which the tanks were lined. I am, Sir,

Your obedient ſervant,

Mr. Taylor. SAMUEL BENTHAM.

A certificate from captain William Bolton, commander of the ſaid veſſel, dated Sheerneſs, 28th of June 1800, accompanied this letter, ſtating, that the water delivered to the Society was taken from a tank holding about ſeven hundred gallons, and which his predeceſſor, captain Portlock, had informed him had been poured into this tank in December 1796, except about thirty gallons added in 1798, and had remained good during the whole time.

The ſignatures to the above accounts were certified on the 28th of June 1800, by the Rev. C. Thee, miniſter of Sheerneſs.

In a letter dated January 27, general Bentham alſo ſtates, that the water which had been preſerved ſweet on board his majeſty's ſloops Arrow and Dart, and of which he had ſent ſpecimens to the Society, was taken from the well at the king's brewhouſe at Weevil, from whence ſhips of war lying at or near Portſmouth are uſually ſupplied with water for their ſea ſtore, as well as for preſent uſe.

III. *Deſcription of a Machine for raiſing Water. By Mr.* H. SARJEANT, *of Whitehaven, in Cumberland* *.

SIR,

I AM ſenſible that the little engine, a drawing of which accompanies this letter, can lay no great claim to novelty in its principle; nevertheleſs it is reſpectfully ſubmitted to the conſideration of the Society, how far its ſimplicity, and cheapneſs of conſtruction, may render it worthy of their attention, with a view to its being more generally known, and uſed in ſimilar caſes.

Irton hall, the ſeat of E. L. Irton, eſq. is ſituated on an aſcent of ſixty or ſixty-one feet perpendicular height; at the foot of which, at the diſtance of about 140 yards from the offices, runs a ſmall ſtream of water. The object was to raiſe this to the houſe for domeſtic purpoſes.

To this end a dam was made at a ſhort diſtance above, ſo as to cauſe a fall of about four feet; and the water was brought by a wooden trough, into which was inſerted a piece of two-inch leaden pipe, a part of which is ſeen at A. (fig. 1. Plate II.)

* From the *Tranſactions of the Society of Arts, &c. Adelphi, London, for* 1801.—The Society awarded their ſilver medal to Mr. Sarjeant.

of

The ſtream of this pipe is ſo directed as to run into the bucket B, when the bucket is elevated; but ſo ſoon as it begins to deſcend, the ſtream flows over it, and goes to ſupply the wooden trough or well in which the foot of the forcing pump C ſtands, of three inches bore.

D, is an iron cylinder attached to the pump rod, which paſſes through it. It is filled with lead, and weighs about 240 lbs. This is the power which works the pump, and forces the water through 420 feet of inch pipe from the pump up to the houſe.

At E is fixed a cord which, when the bucket comes to within four or five inches of its loweſt projection, becomes ſtretched, and opens a valve in the bottom of it, through which the water empties itſelf.

I beg leave to add, that an engine in a great degree ſimilar to this was erected ſome years ago by the late James Spedding, eſq. for a lead mine near Keſwick, with the addition of a ſmaller bucket, which emptied itſelf into the larger near the beginning of its deſcent, without which addition it was found that the beam only acquired a libratory motion, without making a full and effective ſtroke.

To anſwer this purpoſe in a more ſimple way, I conſtructed the ſmall engine in ſuch manner as to finiſh its ſtroke (ſpeaking of the bucket end) when the beam comes into an horizontal poſition, or a little below it. By this means the lever is virtually lengthened in its deſcent in the proportion of the radius to the coſine, about thirty degrees, or as ſeven to ſix nearly, and conſequently its power is increaſed in an equal proportion.

It is evident that the opening of the valve might have been effected, perhaps better, by a projecting pin at the bottom; but I choſe to give an exact deſcription of the engine as it ſtands. It has now been ſix months in uſe, and completely anſwers the purpoſe intended.

The only artiſts employed, except the plumber, were a country blackſmith and carpenter; and the whole coſt, excluſive of the pump and pipes, did not amount to 5 l.

I am, Sir, your humble ſervant,

Warwick Court, Holborn. H. SARJEANT.

Mr. Charles Taylor.

In another letter, dated Whitehaven, April 28, 1801, Mr. Sarjeant further obſerves, that the pump requires about eighteen gallons of water in the bucket to raiſe the counterweight, and make a freſh ſtroke in the pump; that it makes three

three ſtrokes in a minute, and gives about a half gallon into the ciſtern at each ſtroke. He adds, "I ſpeak of what it did in the dryeſt part of laſt ſummer; when it ſupplied a large family, together with work-people, &c. with water for all purpoſes, in a ſituation where none was to be had before, except ſome bad water from a common pump, which has been ſince removed. But the above ſupply being more than ſufficient, the machine is occaſionally ſtopped to prevent wear, which is done by merely caſting off the ſtring of the bucket valve."

P. S. I have juſt been informed that a drawing of the engine, which I had communicated to a perſon in this neighbourhood, was ſent to a colliery near Swanſea ſome months ago, and that it has already been applied to uſe there; it is not, however, ſufficiently powerful for the coal-works in this neighbourhood.

The following Certificate accompanied theſe Letters.

I do hereby certify, that the water engine, conſtructed near my houſe under the direction of Mr. Sarjeant, has been eight months in uſe, and fully anſwers the purpoſe intended.

EDMUND L. IRTON.

Irton Hall, March 18, 1801.

I do alſo certify the above to be true.

ROBERT WILKINSON,
One of his majeſty's juſtices of the peace for the county of Cumberland.

March 13, 1801.

IV. *Obſervations on the* Ipomæa Hiſpida, *and ſome other Plants of the Family of the* Convolvulus. *By* FELIX FONTANA*.

I. THE *ipomæa hiſpida*, and other plants of the *convolvulus* kind, have the property, as they grow up, of twiſting themſelves around bodies in the neighbourhood with which they come in contact. They exerciſe this ſingular property on every kind of body, whether vegetable or mineral, great or ſmall, even when they are as ſlender as a thread.

II. The ſpirals formed by theſe plants are looſe, and at a diſtance from each other, when the diameter of the body which they embrace is of a conſiderable ſize. If it decreaſes they become cloſer, and approach each other more, in ſuch

* From *Mémoires de la Société Médicale d'Emulation. Quatrième année.* Paris 1801.

a manner

a manner that one is ſometimes tempted to believe that they are in contact. I have preſented to theſe plants metals, ſtones, and the branches of trees, ſometimes green and ſometimes dry, the figures of which were infinitely varied, and always obſerved that the ſpirals they form adhere, by immediate contact, to the whole length of theſe bodies. I tried to change the direction of theſe ſpirals, to make them aſcend or deſcend from left to right, if they inclined before from right to left; and they always yielded to theſe changes with facility in a very little time.

III. When they are abandoned to themſelves, and find no bodies near them to which they can attach themſelves ſo as to climb around them, they creep along the ground, fall back on themſelves, become interwoven in a thouſand different ways, and force themſelves into the ground: they extend to a great diſtance on every ſide, and form a net of ſpirals cloſely united.

IV. After having long ſtudied the character of theſe plants, I muſt confeſs that I am acquainted with no mechanical principle by which the movements and ſingular properties which they exhibit can be explained. This kind of inſtinct, which makes them ſearch for neighbouring bodies to aſſiſt them to riſe; thoſe ſpirals, which they conſtantly form as they grow up; thoſe angles and thoſe folds, which are found always proportioned to the figure of the bodies they meet with; the facility with which their ſtems, even when exceedingly hard and robuſt, can fall back upon themſelves, and form angles ſo much the ſmaller as the bodies to which they attach themſelves are ſlenderer: all theſe phænomena, abſolutely contrary to the rectilineal progreſs affected by the fibres of plants, as well as by their veſſels and the liquors they contain, make me ſuſpect, in thoſe which we examine, a new principle of ſenſation and life.

V. It muſt not be believed that the *ipomæa hiſpida* thus twiſts round upon itſelf merely becauſe it is formed of ſpiral fibres, and that all its movements are only a neceſſary effect of its interior organization. Such an opinion is entirely void of foundation. It would indeed be fruitleſs labour to ſearch for theſe pretended ſpiral fibres by the help of the microſcope, for that inſtrument exhibits only rectilineal fibres and canals. Beſides, I have always found by experience that the direction of theſe plants may be changed at pleaſure; that, if the ſpirals they deſcribe around any body are formed on the right, they may be immediately turned to the left, and *vice verſa*. Their movements, therefore, can be aſcribed neither to any peculiar

mechaniſm, nor to local circumſtances; and they ſeem equally capable of moving on all ſides.

VI. Ivy is the only plant, and perhaps the only body, with which they do not ſympathize: they are not fond of uniting with it, or twiſting themſelves around it. If any external power unites them to that plant, and forces them to deſcribe around it a certain number of circumvolutions, they endeavour to avoid it, to diſembarraſs themſelves from it, and they remove from it as ſoon as circumſtances will permit.

VII. If a branch of the plants in queſtion be ſuſpended in the air, it continues to deſcend in a perpendicular direction, unleſs it be very ſlender. In that caſe, it abandons the ſtraight line which it followed, and bends itſelf contrary to the laws of gravitation, raiſes itſelf up, and returns to its own ſtem, that it may deſcribe around it the uſual ſpirals.

VIII. Sometimes the extremity of theſe plants, after having twiſted itſelf ſeveral times around different neighbouring bodies, abandons the ſpirals it had begun to form, and recedes ſeveral inches; eſpecially if deranged in its progreſs, either on purpoſe or by the effect of ſome local circumſtances: but, by a very wonderful mechaniſm, it almoſt always falls back towards thoſe bodies which it embraced, ſearches for them as if by natural inſtinct, and again twiſts itſelf around them, following the ſpirals it deſcribed before. Theſe ſpirals are ſituated in a direction from the top to the bottom, or from the bottom to the top, according to the poſition of the point which terminates them. Indifferent to every kind of direction, they conſtantly follow that given to them, without obeying either the laws of gravity, or thoſe by which other plants ſeem to be regulated.

IX. All theſe phænomena cannot be explained by ſimple mechaniſm: they ſeem to be the effect of a principle of ſenſation and life; a principle which I diſcovered ſeveral years ago in the *tremellæ* of Dillon, and ſome other ſmaller plants, as yet little known, of the family of *liſerons*, and of which I have demonſtrated the exiſtence by evident proofs, as may be ſeen in my different works. Theſe proofs have never yet been conteſted by any obſerver or philoſopher.

X. Life and ſenſation are found to be obſcured among certain animals which have very little analogy with man and with common animals, and to become the leſs apparent as their organization differs from ours. Sometimes, even, it is difficult to believe that they enjoy real life; and this diſcovery can be made only by the laborious reſearches of the philoſophic obſerver. Several of theſe animals are deſtitute of

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the organs of hearing, ſight, taſte, and ſmell. There are whole families which have not the organ of voice, that powerful mean for diſcovering ſenſation; and the ſenſe of touch itſelf is ſometimes ſo obſcure by the abſence of thoſe ſenſible movements and violent agitations manifeſted by other animals in conſequence of any ſtroke, that it cannot be known whether it is produced by ſenſation, or by any other principle unknown, and merely mechanical. Even the organs of digeſtion, which in other animals are internal, can be ſupplied in the family of the polypes by the external organs, and even by the ſkin itſelf. It is well known that theſe animals form a kind of elongated bag, if they are turned, like the finger of a glove; the ſtomach becomes ſkin, and performs the functions of it, while the ſkin becomes ſtomach, and digeſtion continues.

XI. It was formerly thought that, to aſcertain whether an organized being belonged to the animal or vegetable kingdom, it was ſufficient to divide it into ſeveral parts; but at preſent we are acquainted with a multitude of animals which may be divided without deſtroying them, and which even multiply, like ſome plants, in proportion as they are divided.

XII. The little analogy, therefore, which exiſts between our organs and thoſe of plants, and even the abſolute privation of certain organs, will not permit us to obtain certain and evident proofs of the life and ſenſation of vegetables. For, if we ſuppoſe that all plants are endowed with ſenſation, and that it is even much more exquiſite than our own, how can we aſcertain whether their mode of living and feeling be different from ours, if their organs have no relation to our organs, and if they are entirely deprived of thoſe which could act upon ours? But though we cannot prove directly, and in an evident manner, that plants feel, it by no means thence follows that plants are incapable of feeling as well as animals, and even in a much more exquiſite manner. We cannot without temerity, and without expoſing ourſelves to the danger of falling into error, refuſe to nature a power which ſhe exerciſes, perhaps, over all organized beings.

XIII. Motion, perhaps, is the moſt certain character by which animals are diſtinguiſhed, and that which rarely abandons them: without it we ſhould be obliged to conſider as inanimate an infinite number of beings in whom life is manifeſted in the moſt evident manner by the rapid movements of their different parts; movements which can in no manner be aſcribed to mere mechaniſm, but which are neceſſarily produced by ſenſation; and yet theſe movements ſometimes become ſo ſlow, and ſo obſcure, that they are capable of exhauſting

hauſting the patience, the ſagacity, and the talents of the moſt indefatigable philoſophic obſerver.

XIV. But if the vital movements are obſcure in a very great number of animals, they become almoſt imperceptible in plants. I do not here ſpeak of thoſe movements, ſo well known, of the ſenſitive plant, the *hedyſarum gyrans*, and the ſtamina of ſeveral others, which, as ſoon as touched, move and contract themſelves. It would be difficult to prove that theſe movements are really vital, and that they ariſe from ſenſation: they appear to me to be purely mechanical, though we may be ignorant of the real mechaniſm which produces them; and they have a great analogy to thoſe which depend on the elaſticity or ſpring of certain machines which, when once wound up, unbend themſelves, and continue to move as long as the force and principle by which they were put in action exiſts.

XV. The indefatigable botaniſt Dillon ſtudied with great care the *ſmall tremella*, but he did not diſcover thoſe movements which it conſtantly manifeſts to the eye of an attentive philoſophic obſerver. Adanſon went ſtill further, but without obſerving in it any thing that depends on a real principle of life and ſenſation. Having become the object of my reſearches, it exhibited to me a great number of movements ſtill unknown, and of the greateſt importance. After having carefully examined, and, as I may ſay, analyſed them, it appeared to me, as may be ſeen in my works, that they might conduct to a real principle of life altogether different from mere mechaniſm. But I muſt confeſs there are only two plants the life and ſenſation of which I was able to demonſtrate in an evident manner, and ſo as to leave no doubt; and theſe two plants are very ſmall, and even microſcopic. They have little if any analogy with other plants; and though they exhibit all the properties by which vegetables are characterized, ſome even may claſs them in the animal kingdom. But whatever may be the caſe, and however important the queſtion may appear, it cannot deſtroy truths founded on facts and obſervations. It is much to be wiſhed, for the progreſs of ſcience, that the life and ſenſation of common plants could be proved, and that it were poſſible to ſhow that theſe qualities, ſo wonderful, and at the ſame time ſo obſcure, do not excluſively belong to animals, but that they are extended to all organized beings.

XVI. I opened the entrance into this laborious and difficult path by ſome obſervations, which were indeed very imperfect, on the *convolvuli*; a kind of plants which are very large, and well known to every body. Animated ſolely by a deſire

a deſire to be uſeful, obſervers abler than myſelf, and more favoured by circumſtances, will no doubt finiſh what I have only ſketched out.

XVII. To encourage them, and ſhow them the probability of the moſt brilliant ſucceſs, let me be permitted to ſay a few words reſpecting two properties equally common to animals and vegetables, from which life and ſenſation ſeem to flow—I mean generation and reſpiration, ſo neceſſary to theſe two kinds of organized beings.

XVIII. Air is an element ſo neceſſary to animal life, that it becomes weakened, and is at length extinguiſhed, when entirely deprived of it; and from my own obſervations I am induced to believe that no animal can live without air, and without a quantity, more or leſs conſiderable, of oxygen; though we are but imperfectly acquainted with the manner in which it acts, how it is reſpired by the different animals, and by what means they are able to ſeparate it from the different ſubſtances with which it is united, and which can more or leſs alter its purity.

XIX. What is certain is, that ſmall eels, ſuſceptible of being recalled to life, die if deprived of air, or if care be not taken to renew it; and it would be loſt labour to attempt to reanimate them, if the water in which they are immerſed is not in contact with reſpirable air. They live only a very ſhort time in every kind of water deprived of that air. They die in hydrogen and nitrogen; though theſe gaſes are not prejudicial to them of themſelves; and do not occaſion to them ſudden death, like the greater part of the other gaſes, without even excepting carbonic acid gas. It appears, then, that oxygen contains the vital principle; and the principle it has of maintaining and preſerving life, induces us to believe that if it does not communicate it directly, it can at leaſt awaken it, and put it in action. The ſmall animals above mentioned are an irrefragable proof of this aſſertion. They remain dead as long as they are deprived of oxygen, but its preſence immediately reſtores them to life and motion. It may therefore be ſaid that it contains the direct principle of life, which it infuſes into them; or that their vital organs, in order to be reanimated, ſtood in need only of this aliment, of which they were deprived.

XX. The abſolute need which plants have of air in order to vegetate and be preſerved, renders it highly probable that it ſerves for the ſame purpoſes among animals; and that it ought to be conſidered in theſe two claſſes of beings as a principle from which life flows, in the ſame manner as an effect from its cauſe. Beſides, vegetables are organized as

well as animals; they increaſe alſo by nutrition; and though this function in both follows laws different in their details and application, we may however aſſert, that plants have in general more reſemblance to our ſmall eels, than the latter have to common animals. A plant, indeed, may be dried to a certain degree, ſome even may be reduced to a ſtate of perfect deſiccation, without loſing the property of vegetating as before; whereas the ſlighteſt deſiccation of animals is, in general, ſufficient to deprive them of life for ever. The diverſity of theſe phænomena is, no doubt, owing to the ſimplicity of the organs of plants and of our microſcopic eels, and, above all, to the facility with which the latter can be dried, which prevents the corruption and diſſolution of their organs; while this diſſolution is unavoidable among common animals which have a very complex organization, and whoſe humours naturally tend to corruption. This, however, in no manner deſtroys the great analogy which exiſts between theſe two ſpecies of beings; an analogy founded in particular on the circumſtance of both having equal need of oxygen for their exiſtence.

XXI. Generation, that myſtery, ſo obſcure in its principles, which belongs no leſs to plants than to animals, forms the ſecond kind of analogy between the two claſſes of organized beings, and induces us to believe, that where the organs are the ſame, and have the ſame uſes, we ought to find alſo an identity of wants. Vegetables and animals have diſtinct genital parts of ſeparate ſexes, male and female organs in different individuals, and exhibit efforts in theſe different ſexual parts to conſummate the act of fecundation. Since the ſexual organs, and the manner of reproducing themſelves, are common to the two grand families of organized beings, do they differ in ſenſation? And this difference, ſo improbable between beings ſo like in other reſpects, and the only one of the kind, perhaps, that can be found in nature, on what is it founded? Is it on our organs and mode of ſenſation not being in harmony or uniſon with that of plants? Was there ever any reaſon weaker, or leſs philoſophical?

XXII. The movements of the male organs of plants which perpetuate their ſpecies, and the manner in which they prepare for that grand work, ſeem to me to deſerve particular conſideration, and further reſearch. I propoſe to undertake this labour, or rather to continue it; for I have already been employed on this object under various circumſtances, and I am of opinion that the ſum of my obſervations is ſufficiently deciſive to enable me to aſſert, with ſome foundation, that the movements by which different beings repro-

duce themſelves, are not the reſult of mere mechaniſm, but that the grand act of generation is executed by a principle of ſenſation which regulates and directs it: it, however, appears that this ſenſation abandons plants as ſoon as the end of nature is accompliſhed. Thus we ſee different kinds of animals die when they have enſured the reproduction of their ſpecies.

XXIII. It is then highly probable that plants as well as animals enjoy a principle of life and ſenſation, whatever be the nature of that principle, and notwithſtanding the difference it exhibits in theſe two families of organized beings; a difference which is neceſſary, ſince their ſtructure is not the ſame. The abſolute need of oxygen, which is equally felt by both; the ſimilitude of their organs; the analogous movements in the uſe of theſe organs; and, in the laſt place, the inconvenience there would be to refuſe life and ſenſation to other plants, while we are obliged to acknowledge it in the two microſcopic plants which I examined, beſides thoſe alluded to in this memoir; are all proofs on which the life and ſenſibility of plants are founded. It muſt, nevertheleſs, be confeſſed, that the little analogy which there is between their organs and ours, will not permit us to eſtabliſh theſe proofs on facts, and to give them the evidence of demonſtration. It appears, however, very ſurpriſing that nature grants to vegetables a force and an energy which it ſeems to have refuſed to animals. I allude to the property which the former poſſeſs, not only of decompoſing every kind of bodies, but of collecting the elements to form new compounds, and thus to produce foſſils and even metals. Animals are deſtitute of this property, or at leaſt enjoy it only in a weak degree.

XXIV. Life and ſenſibility, however, in different animals exhibit degrees ſo various and different, become weaker, and decreaſe by gradations ſo inſenſible, that the philoſophic obſerver can ſcarcely fix their limits, and aſcertain where they begin and where they end. It even appears that ſenſibility, to judge at leaſt by its effects, is not altogether of the ſame nature in the different kinds of animals. The numerous obſervations and experiments, which I have made for years, on the ſenſibility of animals, particularly the cold-blooded, and on the return of ſome of them from a ſtate of death to that of life, leave no doubt that there are a great many of them in which the ſevereſt wounds occaſion neither ſenſation nor pain. For example, if the head of a fly* be

* The naturaliſt and real philoſopher will not be ſurpriſed to ſee the author fix his attention on a fly, an inſect conſidered by the vulgar as contemptible. The ſmalleſt inſect enjoys life and ſenſation as well as the

be cut off, it continues to fly about, to walk, to leap, and to climb as before; its regular and uniform motions, ſometimes ſlow and tranquil, ſometimes quick and animated, exhibit nothing convulſive, nothing that reſembles thoſe violent agitations which accompany pain. In a word, the number and rapidity of theſe movements, as well as the time that theſe flies can live, are ſubject to no fixed rule.

Let us ſtop for a moment to conſider the movement of their legs; they are equally various and wonderful as thoſe by which they can move the whole of their body.

XXV. To appreciate properly the different movements performed by an animal in a ſtate of health or of diſeaſe, it is neceſſary to have an exact idea of the circumſtances in which it is placed, of the motives and impulſes which in general induce it to move; in a word, we muſt be acquainted with its nature and character. This being premiſed, it is eaſy to obſerve, that of the nine kinds of movement to which thoſe executed by flies in a ſtate of health may be reduced, eight ſtill ſubſiſt even after they have been deprived of the head. Scarcely has a fly been decapitated when it is ſeen to move its hind legs for a long time, and at different periods to croſs them, either ſtanding up or flying, to rub them along its body, to hook the one into the other in the form of a wheel, and to agitate them in a thouſand ways with wonderful rapidity and regularity. All theſe movements, which conſtitute the firſt kind here treated of, are abſolutely ſimilar to thoſe performed by a fly, when, poſſeſſing perfect health, and being haraſſed by no fear or want, it appears to move only for its own pleaſure. Theſe movements, then, ſeem to be produced in theſe two ſtates, ſo oppoſite, by the ſame principle, that is, ſenſation and volition.

XXVI. The ſecond kind of movement reſults from their hind legs, which they raiſe up towards the interior edge of their wings; ſo that by theſe means they are elevated, and placed in different directions. The object of theſe movements, exceedingly varied, though conſtant, ſeems to be, to rub, beat, and poliſh their wings. Their hind legs can execute a third kind of movement, no leſs varied, when, conveying them above their wings, which they keep extended, they rub them, beat them, and poliſh them, as in the preceding caſe. Their middle legs alſo are often obſerved to be twiſted around thoſe behind, and to rub each other mutually; which forms two new kinds of motion different from thoſe we have examined.

largeſt animals: theſe properties belong as much to microſcopic and infuſion animals, as to the elephant and whale. Nothing is contemptible in the eyes of the philoſopher; every thing in nature is great and wonderful.

XXVII.

XXVII. They can alſo move their fore legs, and twiſt them in the form of a wheel; which forms a ſixth kind of movement, ſo various and multiplied that the eye can ſcarcely follow them. The ſeventh and eighth kinds reſult from the union and interlacement of their middle legs with thoſe before. What ſeems ſurpriſing is, that theſe eight kinds of movement are perfectly ſimilar to thoſe voluntarily performed by the animal, and as it were for pleaſure, before its head is cut off, and when it enjoys perfect health and tranquillity. In the laſt place, flies perform a ninth kind of movement by conveying their legs and thighs around their head, which they rub and poliſh in various directions. It is evident that this movement cannot take place after they have been decapitated.

XXVIII. Flies certainly would not exhibit theſe phænomena if they experienced as acute pain as warm-blooded animals when ſubjected to decapitation, which is conſtantly followed by ſpeedy death. Will it be ſaid that theſe different movements ſuppoſe, on their part, neither pleaſure nor recreation, but rather a need which they experience of rubbing and poliſhing their legs, their wings, and different parts of their bodies? But if this be the caſe, we muſt be forced to confeſs that decapitation does not prevent them from being ſenſible to ſmall privations and old habits, which are by no means neceſſary to their exiſtence; a manifeſt proof that it occaſions no pain to them, or at leaſt that it is exceedingly ſlight; otherwiſe they would not attend to things of ſo little importance.

XXIX. Cold-blooded animals could ſupply me with many examples of a ſimilar kind; but I ſhall content myſelf with mentioning only one, furniſhed by the tortoiſe. If the head of this animal be cut off, or, what is ſtill better, if its brains be ſcooped out with dexterity, it ſeems to be ſcarcely ſenſible of the operation; it continues to walk as before, it aſcends and deſcends, avoids thoſe obſtacles which it meets with, turns itſelf to different ſides, ſtands up on its hind paws, and performs movements of every kind; and though its paws are covered with a hard and ſcaly ſkin, if they are ſlightly touched even with a feather when it is walking, or has been a little irritated, it immediately draws them back, and remains ſome time at reſt: in a word, its ſenſibility ſeems to be ſo little altered, that it feels the ſmalleſt impreſſion made on its ſhell. Tortoiſes live in this manner five or ſix months without any change being obſerved in their movements or habits: there are even certain movements which they execute with more readineſs and ſurety than in their ſtate of integrity. Such is

that

that by which they turn themſelves on their belly, and ſtand up on their hind paws. They at length die at the end of ſix months; but it appears that their death ought to be aſcribed rather to hunger than to the operation to which they have been ſubjected, ſince thoſe on whom it has not been performed die in the ſame time if deprived of nouriſhment. If the tortoiſe in this ſtate experiences real pain, were it even very ſlight, it would certainly not feel the weak impreſſions made on the ſhell which covers it; for it is well known that a very ſtrong impreſſion entirely effaces that which is very weak.

XXX. The facts I have eſtabliſhed will, no doubt, find many oppoſers. This muſt be the caſe. They are ſurpriſing; have even ſomething of the marvellous; and tend, in particular, to deſtroy errors hitherto conſidered as inconteſtable truths.

XXXI. We, however, know that ſenſibility is very weak in animals at the time of their birth; that it is gradually expanded with their organs; and, when come to its higheſt degree, it begins to decreaſe by inſenſible gradations until it is at length entirely extinguiſhed, when the animal dies. It is eaſy to obſerve theſe different degrees of ſenſibility in animals attacked by diſeaſes, or ſubjected to experiments of this kind.

XXXII. I might here add, that no naturaliſt is now ignorant that the ſmall eels which have been the object of my obſervations, and which are found in different kinds of the gall-nut, can paſs inſenſibly from the ſtate of death to that of life; die again, to be afterwards revived, if circumſtances admit; and that the number of theſe different reſurrections is ſtill indetermined. It is more than twenty-ſix years ſince I publiſhed, in different journals, the obſervations which confirm theſe truths; and I have demonſtrated, in the moſt evident manner, that theſe ſmall animals may be revived at pleaſure; that they enjoy this ſingular faculty as long as they are hermaphrodites, but that at the moment when they aſſume a ſex they return to the common law, and, when once dead, can no longer be revived. I have even found the means of giving them a ſex at pleaſure, and theſe means are thoſe employed by nature for perpetuating their ſpecies*.

V. *An*

* Theſe truths and ſeveral others are explained and illuſtrated in a work, which I intend to publiſh, on the life, death, and ſenſibility of animals. This work, ſeveral fragments of which have been communicated, for more than twenty years paſt, to different learned men in Europe, will be enriched with above 200 engravings, and will form two large volumes

V. *An Examination of* C. CLOUET'S *new Process for making Cast Steel from Bar Iron by means of the Decomposition of Carbonic Acid.* *By* DAVID MUSHET, *Esq. of the Calder Iron Works* *.

IN the present extensive state of the iron trade in Britain, which exceeds in that manufacture alone the collective exertions of all Europe, every discovery which may lead to elucidate the principles of this art, and place them upon a permanent foundation, or which may remove those local disadvantages, the inevitable consequence of the use of pit-coal, become highly interesting. To counteract the inferior effects produced by this otherwise invaluable combustible in the conversion of cast into malleable iron, and to produce British bar iron equal in quality for all purposes, steel-making included, to the foreign fabrics, would be an attainment of much national importance. Multiplied beyond precedent have been the modes devised within the last fifty years; but as they have been merely modifications of each other, with no essential change of principle in the operation, the results have been consequent to the unvaried nature of the means. Every new idea, therefore, ought to be seized with eagerness, accurately examined, and put to the test of experiment. Ultimately great improvement may be drawn from a hint loosely thrown out, or even derived from experiments, which, though at first sight engaging, may neither carry with them conviction of principle, nor be productive of eventual utility. Impressed with a full conviction of these truths, the process for converting iron into steel, communicated by the French chemists in the *Journal des Mines*, No. 45, appeared to me deserving of a fair examination. To this process I alluded in my last communication, and to it I shall now particularly attend.

The report of citizens Guyton and D'Arcet to the French National Institute, announcing this discovery, sets forth, that C. Clouet, among other inventions, had devised a method of converting iron into steel by means of the decomposition of the carbonic acid in lime. To prove this,

lumes in quarto. I have need only of a few moments of philosophic leisure to put the last hand to it; but, can I hope for it after having been persecuted with as much injustice as barbarity?

* Communicated by the Author.—In Mr. Mushet's paper given in our last Number, Vol. xi. p. 289, line 16, for *slate* of steel, read *shale* of steel.

several

ſeveral experiments are detailed with a degree of minuteneſs bordering upon demonſtration.

The report ſtates, that a $\frac{1}{3\frac{1}{2}}$d part of carbon is ſufficient to convert iron into ſteel: a quantity of it, equal to $\frac{1}{8}$th of the weight of the iron, gives a quality of ſteel more fuſible but ſtill malleable; but beyond that term it approaches to caſt iron, and has no longer ſufficient tenacity. By increaſing the doſe of carbon you increaſe the fuſibility, and it paſſes at length into the ſtate of gray caſt iron.

In purſuance of the proportions here laid down, we find the report preſcribes, that a mixture of carbonate of lime and argil, compriſing 2-3ds the weight of the iron, muſt be employed, viz. 1-3d the weight of the iron of carbonate, and the ſame proportion of argil. Theſe are to be introduced into a crucible along with fragments of iron, expoſed to heat for a time ſufficient to effect fuſion, and that the reſult will be found ſteel.

That C. Clouet applied the proportion of 1-3d of carbonate of lime, from a calculation of the quantity of carbonic acid it contained, and thence the quantity of carbon diſengaged by its probable decompoſition, will appear very evident from an analyſis of the experiments performed by citizens Guyton, D'Arcet, and Vauquelin.

The quantity of iron operated upon in this experiment was 6914 grains. Admitting, according to Clouet, that $\frac{1}{3\frac{1}{2}}$d part of carbon was ſufficient to convert this quantity into ſteel, then 216$\frac{6}{3\frac{1}{2}}$ grains of carbon are requiſite for this purpoſe. We find that one-half of the flux uſed amounted to 2304 grains of carbonate of lime. If we ſuppoſe this united with 36 *per cent.* of carbonic acid, then the reſulting quantity of acid will be 829·44 grains. Granting, according to Lavoiſier, that the component parts of carbonic acid are 72 of oxygen and 28 of carbon, then by the following formula we have $\frac{829{\cdot}44 \times 28}{100} = 232{\cdot}5$ grains of carbon, nearly.

	Grains.
Contained in this portion of lime	232·$\frac{16}{3\frac{1}{2}}$
Quantity of carbon neceſſary to ſteelify the iron at the rate of $\frac{1}{3\frac{1}{2}}$d part	216·$\frac{6}{3\frac{1}{2}}$
	16·$\frac{10}{3\frac{1}{2}}$

This ſmall difference is eaſily accounted for by ſuppoſing the leſs difference to have exiſted as to the quantity of carbonic acid contained in the lime uſed by Clouet.

I think it probable, from the coincidence here pointed out, that the other proportions of Clouet were fixed by the ſame ſtandard of calculation; at leaſt, the detail of experiments which

which I ſhall bring forward relative to the various parts of this theory, as formerly ſtated, will bear certain evidence that the whole had been founded upon one ſeries of experiments, the juſtneſs of which may be gathered from what follows.

Grains,

Exp. I. Four pieces of malleable iron, weighing 1457 were introduced into a Stourbridge clay crucible, and the following mixture added, being the exact proportions pointed out by Clouet:

1-3d of Balgrochan raw lime, or 486 grains;

1-3d of Stourbridge clay (old pot), or 486 grains.

The pieces of iron were imbedded in, and completely covered by, the mixture of earths. The crucible was expoſed for 35 minutes to a high heat, when the mixture was ſuppoſed to be reduced. The reſult was a fine dark green glaſs, tranſparent towards the edges, covering a perfect regulus of metal. Upon cloſe examination, a few traces of imperfect cryſtallization were viſible upon the upper ſurface. The under ſurface was marked with deep pits or honey-combs, but nothing which betokened any great degree of diviſion having taken place while the metal was fluid.—This product weighed - - - - 1437

Loſt in fuſion 20

equal to $\frac{1}{73}$d part the weight of the iron introduced. From the dark colour of the glaſs I inferred that a quantity of iron equal to the deficient weight might eaſily be diffuſed throughout the maſs.

The quality of this metallic button was minutely examined, and the following characteriſtics rigidly aſcertained:

The maſs was partially cut acroſs with a chiſel, and with difficulty broken cold over an anvil. The fracture diſplayed a conſiderable quantity of fibre; a few imperfect granulations, of no determinate form, were alſo viſible, but poſſeſſed of a ſmall degree of comparative luſtre. One-half of the button was forged at a cherry heat. It drew into a ſquare ſhape with facility; but the action of the hammer, even at this low heat, expreſſed a conſiderable number of ſparks. The ſolidity of the bar, notwithſtanding the fuſion, was far from being complete. At a high red heat, a ſlight hammering diſſipated the particles of metal like caſt iron.

One end of the bar was plunged into water at the ordinary heat for ſteel. This produced no alteration, and the bar bent ſeveral times without breaking. When again hammered, it was plunged at a bright red: the ſurface then ſhaled a little,

and, upon being touched by the file, felt perceptibly harder. The fracture of the piece thus hardened was of an open regular grain, approaching somewhat, in point of colour, to malleable iron. A fracture from the unhardened part of the bar was precisely that of bar iron made in the puddling process; a dark blueish fibre breaking short across, without tearing out in groups. But before I allowed myself to draw any conclusion from this experiment, I deemed it necessary to repeat it as follows:

Exp. II. The quantity of iron now operated upon was	Grains. 1331
To which were added 1-3d of the same carbonate of lime, or 443 grains; 1-3d of Stourbridge clay (old pot), 443 grains.	
This mixture was arranged as in No. 1. The crucible was introduced into the furnace, and in 30 minutes I judged the fusion entire. A metallic button, as in No. 1, was found, beneath a dark green glass, which weighed	1312
Lost in fusion	19

equal to $\frac{1}{70}$th part of the original quantity of iron introduced.

The glass obtained in this experiment, upon comparing fragments of equal thickness, was found one shade lighter than in No. 1. The upper surface of the metallic button exhibited a more perfect approach to crystallization than in No. 1. This supposes a more perfect fusion had been effected. The under surface contained one large pit, the interior surface of which was strongly marked with a streaky crystallization. The fracture was also brighter, and the grain more distinctly formed than in No. 1.

A bar forged from one-half of the button drew into a shape at a low red heat. At a white heat it stood a few blows; but, in returning through the intermediate heats, dissipated like hot sand. A fragment well hammered, and plunged into water at a low red heat, exhibited no shale, and bent easily over the anvil. When plunged at a bright red heat, a fair proportion of shale turned up. The piece then felt similarly hard, under the file, with No. 1. The hardened fracture was open, distinctly granulated, and bright. A fracture of the unhardened part of the bar was blueish, tough, without grain; nearly two shades lighter in the colour than No. 1. This circumstance, along with others formerly mentioned, was most probably derived from the superior degree of fusion which the product underwent.

The same experiment was several times repeated with nearly

nearly ſimilar reſults as to loſs of iron. The quality of the metal was found invariably the ſame. I therefore deemed myſelf warranted to draw the following concluſion, that with the quantities of mixture preſcribed by the French chemiſts for converting iron into ſteel, I had not been able to obtain a product which, under the various teſts, exhibited the properties of caſt ſteel. But, from the reſult of the foregoing experiments, I did not think myſelf at liberty to infer, that although the uſual marks of ſteel had not been perceptible, ſteel could not be formed in this way. Aware that carbonates of lime differ much in their reſpective quantities of carbonic acid, and proceeding upon the ſuppoſition that the carbonate uſed by C. Clouet might afford a greater quantity of carbonaceous matter, I reſolved to make up in quantity of earth what probable deficiency might have ariſen from an inferior quantity of carbonic acid.—To aſcertain this point, I made the following experiment:

	Grains.
Exp. III. I uſed of ſmall pieces of iron -	1342½

To which were added ½ of carbonate of lime, or 671¼ grains; ½ of Stourbridge clay pot, 671¼ grains.
The quantity of carbonate here uſed exceeded that employed by Clouet in the proportion of 9 to 6, and ought to have given out to the iron, every thing elſe being alike, a portion of carbonaceous matter equal to $\frac{1}{21\frac{4}{6}}$ths the weight of the iron employed. This mixture was fuſed in 38 minutes, and, as uſual, a metallic button found under the fuſed earths which weighed - 1325½

Loſs equal to $\frac{1}{79}$th part of the weight of the iron fuſed 17

The glaſs appeared to be a minute ſhade lighter than No. 2. The upper ſurface poſſeſſed a few imperfect radii, a commencement of the uſual cryſtallization. The under ſurface was ſimilarly pitted with No. 1 and 2; and no alteration of quality could be inferred from the external character of this button. The fracture of it when broken was entirely fibrous, and tore aſunder ſomewhat like lead. Under the hammer it cracked leſs than did the former two. It was next double-welded, and drawn a ſecond time; cracking a little, and throwing off a few ſparks. In hardening, and in every ſtage of the ſubſequent examination, I could detect no circumſtance that would warrant the ſuppoſition of one additional atom of carbon being in combination with this iron beyond that poſſeſſed by Nos. 1 and 2. The property of its ſtanding a higher heat, and exhibiting a tougher fracture, are circumſtances rather the reverſe, but which a more attenu-

ated

ated or less perfect fusion will, I conceive, satisfactorily account for.

In order further to elucidate this subject, now pregnant with doubts as to the efficacy of the carbonic acid,

Exp. IV. was performed as follows: Grains.

Iron introduced		1468
Carbonate of lime	930 grains,	
Stourbridge clay pot	930 grains:	

this proportion being of each of the mixtures 5-8ths the weight of the iron, and to C. Clouet's proportion as 6·25 to 3·33; nearly double. From this mixture, still following his calculation, should have been obtained steel, approaching to cast iron in point of saturation of carbon.

The fusion was completed in 32 minutes, and a very perfect crystallized button of metal was produced, which weighed - - - - 1434

Lost in fusion 34

equal to $\frac{1}{44}$th part of the original weight of iron introduced.

The glass now obtained was darker by a marked shade than that obtained in experiment No. 1. This circumstance, and the increased quantity of glass, may in some measure account for this experiment losing more iron than any of the former. The external figure and density of this metallic button was in no way altered from the former results. It underwent the same minute examination as did buttons Nos. 1 and 2, and in no instance afforded the least perceptive difference in point of quality.

Exp. V. was performed as follows: Grains.

Iron introduced		1307
Carbonate of lime	1960 grains,	
Stourbridge clay pot	1960 grains.	

This quantity exceeded that of C. Clouet's in the proportion of 3 to ·666, and of course contained nearly five times the quantity of carbonic acid. This ought to have afforded a result in gray cast iron.

The fusion was completed in 30 minutes, and a metallic button, as usual, obtained decumbent to a very ponderous mass of glass, and weighed - 1275

Lost in fusion, equal to $\frac{1}{40}$th part of the weight of the iron, 32

The glass obtained in this experiment, upon comparison, was found to be exactly the same with No. 1. The great deficiency of weight, in the proportions of 40 to 73, may be accounted

accounted for by taking into conſideration the extra quantity of earths introduced, and fuſed in contact with the iron. The vitrid maſs had imbibed as large a ſhare of the colouring principle, from whatever ſource derived, as that in the firſt experiment, where not above 1-5th part the weight of the earths were uſed to a nearly ſimilar portion of iron. The metallic button now produced was ſubjected to the ſame teſts with the former, and reſults exactly ſimilar in every ſtage of the examination were obtained.

The reſults of the foregoing experiments, I conceive, will with ſafety admit of the following deductions:

That in treating malleable iron with any of the foregoing proportions, of an equal mixture of carbonate of lime and ſome vitrified Stourbridge clay-pot, reſults in fuſion are obtained the qualities of which differ ſo little from each other, that no change ſeems to be derived from altering the proportions of the earths preſcribed: That in no ſhape nor form does iron in this ſtate approach to what is commonly underſtood to be ſteel; nor can it, in any caſe where elaſticity, edge, and poliſh, are requiſite, be uſed as a ſubſtitute for it.

It might, however, prove a curious ſubject of inquiry, and in the end be productive of conſiderable utility, to inveſtigate the principle of alteration that takes place in this proceſs. The iron loſes in weight from $\frac{1}{40}$th to $\frac{1}{79}$th part, depending moſt probably, in ſome meaſure, upon the quantity of earths uſed. If the deficiency conſiſts of iron alone, there can be no reaſon aſſigned why the reſulting button of metal ſhould be ſo completely altered in its properties. This becomes deprived of its malleability when heated beyond a bright red, and particularly of the property of welding. It acquires additional ſoftneſs when cold; and in that ſtate will flatten aſtoniſhingly, without rending, or exhibiting the ſmalleſt crack.

C. Clouet, in fuſing iron and glaſs, attributes to the latter the property of combining with the former. Were a combination of earths with iron ſuppoſed in the preſent caſe, this would naturally add to the weight of the product, unleſs a greater quantity of iron united to the glaſs than left the glaſs to unite with the iron. Again, this ſuppoſition involves the exertion of a double affinity, wherein a preference of predifpoſing cauſe cannot be attributed to the one more than to the other. It will therefore remain for experiment to decide whether the alteration in quality of the iron is occaſioned by an aſſumption or a loſs of principle.

Exp. VI. Before I finiſhed this part of the examination of

C. Clouet's proceſs, I deemed it of importance to make the experiment that follows:

	Grains.
Iron introduced - - -	1399
Carbonate of lime, the ſame as formerly uſed, 1399 grs. From this mixture, by fuſion, was obtained a very denſe maſs of iron, poſſeſſed of a ſmooth radiated ſurface, different in every external feature from the reguli obtained in the former experiments. It weighed	1317
Loſt in fuſion, equal to $\frac{1}{17}$th part the original weight,	82

This extra loſs of metal, however great, appeared ſtill more ſo upon breaking the button, and detecting in its interior a conſiderable portion of incorporated glaſs. From the apparent quantity found in this ſtate it may be fairly preſumed that $\frac{1}{15}$th part the weight of the iron employed had diſappeared in the fuſion. The glaſs, in point of colour, was green, approaching to black; its fracture, in point of luſtre, reſembled the poliſh of fine black marble.

Upon ſubjecting part of the metal now obtained to the hammer, it was found ſoft in the extreme when cold, but in no ſtate would ſtand the effect of ſlight hammering beyond a low red heat. A piece beat out, heated pretty high, and plunged into water, turned up a rich and uniform ſhale, ſo as to make its ſurface reſemble tin; but the ſame, when attempted to be broken, bent ſeven times before it parted. The grain then, far from reſembling the cloſe uniform grain of caſt ſteel, preſented a ſilky fibrous appearance. In this reſpect it differed materially from the reſults obtained in the five former experiments. A bar, $\frac{5}{16}$ths ſquare, plunged hot into water, and ſuffered to cool ſlowly, had aſſumed a partially cryſtallized fracture, reſembling ſome varieties of American red-ſhort bar iron. In ſeeking for a ſolution of the phænomena which appeared in this experiment beyond thoſe in the former, it may be ſuppoſed, upon the principles of the theory advanced by C. Clouet, that the ſuperior denſity of the button was in conſequence of a greater degree of diviſion in the fluid: that this, in part, might be occaſioned by the oxygen ſet free by the decompoſition of the carbonic acid: that part of this oxygen might ſeize upon the iron, and, while theſe conveyed an unuſual degree of colouring principle to the glaſs, might at the ſame time account for the great deficiency of metal.

This ſubject, of the greateſt importance to a juſt theory of the component parts of the various modifications of iron, will meet a copious inveſtigation hereafter. Suffice it for the preſent

preſent to draw the following concluſion, warranted by the foregoing experiment: That when equal portions of iron and carbonate of lime are fuſed together, a metallic button is the reſult, even leſs allied to ſteel than the products of experiments Nos. 1, 2, 3, 4, 5.

The iron uſed in theſe experiments was drawn down from a bar of excellent Swediſh iron, a fragment of which was always put in competition with the products obtained by fuſion. In theſe, the only appearances that indicated ſteel were, ſhaling, and ſlightly hardening when plunged hot into water. Both of theſe properties were, however, much more conſpicuous when a piece of the unfuſed bar was ſubjected to the ſame teſts. In ſhort, all good foreign bar iron, when ſimilarly treated, is ſuſceptible of the ſame diſplay of properties.

It may be here proper to obſerve, that the carbonate of lime uſed in theſe experiments was the ſame throughout; that, after being dried in a temperature of 180° of F. it yielded in diſtillation 39 *per cent.* of carbonic acid.

VI. *On the Strength of Acids, as indicated by the Specific Gravity, and by the Areometer of M. Baumé.* *By* Robert Bingley, *Eſq. King's Aſſay Maſter, Mint, London.*

To Mr. Tilloch.

SIR, Tower, Jan. 23, 1802.

AS I do not remember to have ſeen any table ſhowing the correſpondence between the ſpecific gravities of acids and the degrees of the *peſe-liqueur* (areometer) of Baumé, and as I am poſſeſſed of a good inſtrument of Baumé's, and have examined the nitric acid of different ſpecific gravities, and made a compariſon with the above inſtrument, I am enabled to give a ſufficiently accurate idea of what ſtrength of acid is meant when Baumé's areometer is quoted. I remember, a few years ago, being much at a loſs to underſtand the power of acids expreſſed by the French chemiſts according to Baumé, and made many inquiries amongſt our chemiſts without obtaining the information wiſhed for; and as the experiments made on the continent are generally deſcribed according to the *areometre de Baumé*, and much read in this country, I have annexed a table of thoſe various degrees of nitric acid which have been examined and compared with the French inſtrument; and if you think them worthy a place in your Philoſophical Magazine, you are welcome to inſert them. It is not pretended that this ſcale is accuracy itſelf, though I am confident it will be found ſufficiently ſo for moſt purpoſes; and

it is surely very essential to have a clear idea of the power and strength of an acid used in various metallic solutions.

This comparison was made in September last, and the temperature about 60. Having a stock of acid of the specific gravity 1435, a little distilled water was daily added, and the gravity taken the day after each addition of the water; then the *areometre de Baumé* was immersed with the following correspondence. It will be easy to understand and calculate the degrees of Baumé both above and below those already compared and examined. I had no acid of higher gravity than 1435, nor did I descend lower than 1150; which is a weaker acid than used in most metallurgic operations. The areometer, or *pese-liqueur*, is graduated from 0 up to 50.

Specific Gravity. Water taken as 1000.	Strength by the Areometer.
1435	45
1416	43
1400	42
1383	41
1367	40
1358	39
1350	38
1342	37
1333	36
1312	35
1300	34
1283	32
1275	31
1267	30
1250	29
1233	28
1216	26
1167	20
1150	18

VII. *Observations on the Plant by which the Indians of America preserve themselves from the Bite of venomous Serpents. Communicated to Count* RUMFORD *by Don* PEDRO D'ORBIES Y VARGAS *.

THE abundance of venomous serpents found in the warm districts of America has rendered it necessary for the

* From the *Bibliothèque Britannique*, by Professor Pictet.

unfortunate

unfortunate Indians and negroes, who traverſe the woods almoſt always barefooted, to ſearch out the moſt efficacious remedies for the diſagreeable effects produced by the bite of theſe animals. Of the remedies hitherto diſcovered, none is equal to the juice of a plant of the creeping kind called *vejuco du guaco*; for it not only cures the maladies ariſing from the bite of ſerpents, but preſerves from theſe effects thoſe who have drunk of it before they are bitten; ſo that the negroes and Indians acquainted with this plant, lay hold, with their naked hands, of the moſt venomous ſerpents without ſuſtaining any injury from them. This knowledge, of which they formerly made a great myſtery, gave them much importance in the country; and there is no doubt that they gained a great deal of money, both from thoſe who were bitten by ſerpents, and from thoſe who were deſirous, through curioſity, to ſee them handle theſe dangerous animals.

Being born in the kingdom of Santa-Fé, belonging to South America, I had often heard the inhabitants boaſting of the great ability of theſe negroes, whom my countrymen call *empirics*. But as in the capital, where I was educated, which lies in a cold diſtrict, there are no venomous ſerpents, I had no opportunity of ſeeing any till the year 1788, when, being at Margerita, I heard of a ſlave who had a great reputation as being invulnerable to ſerpents, and who belonged to a gentleman of that place. As I was reſolved to examine him myſelf, I begged his maſter to ſend for him, with a ſufficient proviſion of ſerpents; which he readily conſented to do.

On the 30th of May, the ſame year, the negro came to the houſe where I reſided with one of the moſt venomous ſerpents of the country, which he had put into a calabaſh; a kind of veſſel employed by theſe people for the ſame purpoſes as bottles are employed in Europe. Having informed him that I was deſirous of ſeeing a ſpecimen of his talents, he replied that he was ready to gratify my curioſity, and, taking the ſerpent from the calabaſh, handled it with ſo much confidence and compoſure, that I imagined he had previouſly deprived it of its teeth that contained the poiſon. I therefore cauſed him to open its mouth: but I ſaw that it ſtill had its teeth; and was convinced that the negro poſſeſſed ſome ſecret for ſoothing it, for it appeared as tame and harmleſs as the moſt innocent animal could have been. After a long converſation with the negro, of whom I aſked ſeveral queſtions, to which he gave the moſt pertinent anſwers, I informed him how much I ſhould be gratified if I could be enabled to handle ſerpents with the ſame ſecurity; and, find-

ing that he was not averſe to procure me that ſatisfaction, I offered him a recompenſe, with which he ſeemed ſatisfied. Next morning he returned with the leaves of the plant in queſtion, which he moiſtened, and, having bruiſed them in my preſence, made me drink two large ſpoonfuls of the juice.

He then made three inciſions between my fingers in each hand, in which he inoculated me with the ſame juice: he performed a ſimilar operation on each foot, and on each ſide of my breaſt. When theſe operations were finiſhed he informed me that I might lay hold of the ſerpent. I made ſeveral obſervations to him in regard to the diſagreeable conſequences to be apprehended in caſe I ſhould be bit by the animal; but finding that he ſeemed confident in his ſkill, I reſolved to take it into my hands without any fear; which I did ſeveral times, the animal never making the leaſt attempt to do me any injury. One of the individuals, however, who were in the houſe, being deſirous to run the ſame riſk, was bit by the ſerpent the ſecond time he took it in his hand; but without any further inconvenience than a ſlight inflammation in the part.

Two of my domeſtics who had been alſo inoculated, encouraged by this firſt attempt, went out into the fields and ſoon brought with them another kind of ſerpent, equally venomous, without ſuſtaining any hurt from it. In a word, I have caught ſeveral ſince that time without any other preparation than that of having drunk a little juice of the *vejuco du guaco*; and after repeating theſe trials, either on myſelf or my domeſtics, and always with the completeſt ſucceſs, I reſolved, in 1791, to give a memoir on this remarkable antidote in a periodical paper publiſhed every week at Santa-Fé. I added a deſcription of the plant, and every thing that appeared to me neceſſary for rendering public and general this diſcovery ſo uſeful to mankind. An account of all my experiments, and of the perſons who were preſent, will be found in that paper, dated Sept. 30, 1791.

I ſhall here only obſerve, that the tradition current among the Indians and negroes of the vice-royſhip of Santa-Fé, reſpecting the manner in which the virtue of this plant was diſcovered, is as follows:—A bird of the kite kind, deſcribed by Cateſby under the name of the *ſerpent-hawk*, feeds chiefly upon ſnakes in the hot and temperate regions of that part of America. This bird has a monotonous cry, ſometimes very diſagreeable by its repetition, which imitates the articulated word *guaco*, on which account the inhabitants have given it that name; and theſe people ſay, that when it cries it is to call forth the ſerpents, over which it exerciſes a certain kind

of

of authority. They add other extravagant fables: but it is certain that the *guaco* purſues them wherever it finds them; and that the Indians and negroes, who ſpend the greater part of their time in the foreſts and open fields, aſſert, that to take them with more ſafety they prepare themſelves by eating ſome leaves of the plant in queſtion. This may be true; they may have diſcovered the virtue of it, and experienced it with ſucceſs. In this caſe, as in many others, the inſtinct of animals has been of uſe to us.

In regard to the plant, its genus has not yet been claſſed in any book of botany I have ever ſeen; and for that reaſon I ſhall venture to give a deſcription of it as well as I can, taking advantage of the memoir above mentioned. The root is fibrous, and extends in every direction. The ſtem is ſtraight, perfectly cylindric when the plant is tender, but when old becomes pentagonal, that is to ſay, acquires ſalient angles. The leaves which grow on the ſtem ſtand oppoſite to each other; are ſhaped like a heart; have a dark green colour intermixed with violet; are ſmooth on the lower ſide, rough on the upper, and ſomewhat velvety: its corymbiferous flowers are yellow, floſculous, and have four fleurons in each common calyx. The corolla is monopetalous, infundibuliform, with five indentations; and contains five ſtamina, united by antheræ in the form of cylinders, which embrace the ſtyle. The ſtyle has a ſtigma deeply divided, and the calyx contains ſeveral broad ſeeds, each with a ſilky aigrette.

The plant is vivacious, and is found in the hot and temperate regions of the vice-royſhip of Santa-Fé; it is, in general, fond of growing on the borders of rivulets and in ſhady places, rather than in the open plains. Nature has not produced it in the elevated or cold diſtricts of this continent; and for this reaſon, no doubt, that its virtue would be uſeleſs, as there are no venomous ſerpents but in the countries where it grows.

VIII. *Account of a ſingular Method of hunting Wild Swine in the Iſland of Sumatra. By Mr.* JOHN, *Miſſionary at Tranquebar* *.

Tranquebar, Feb. 10, 1797.

IN the kingdom of Siak, in the iſland of Sumatra, which lies oppoſite to Malacca, there are two kinds of wild ſwine: one kind live in the foreſts on roots and fruit; their fleſh is

* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. ii.

exceedingly well tafted, but they are not numerous. The other kind frequent the impenetrable buſhes and marſhes on the ſea coaſt, where they live on crabs and roots. They are not ſo large as the European ſwine, are of a grayer colour, and keep in large herds. The latter, at certain periods of the year, in herds of ſometimes a thouſand, ſwim from the one ſide of the river Siak to the other, at its mouth, which is three or four miles in breadth, and return to the former at ſtated periods. This paſſage takes place alſo in the ſmall iſlands, by their ſwimming from the one to the other. On ſuch occaſions, theſe ſwine are hunted by a tribe of the Malays, who are not Mahometans but heathens, and who are held in great contempt by the other Malays, who will neither ſuffer them to inhabit among them nor to approach them. On this account they were obliged formerly to live more at ſea than on land, in ſmall boats covered with mats, until the king of Siak lately permitted them to eſtabliſh themſelves in the uninhabited diſtricts on the ſea coaſt. They are called in the Malay language the *Salettians*, and by the Dutch the *Lard Malays*. They are better made and of a brighter colour than the other Malays; have only one wife each, whereas it is well known that the other Malays admit of polygamy.

The women are ſaid to be remarkably well made, and to retain their charms much longer than thoſe among the other Malays and inhabitants of the ſea coaſt; ſo that at the age of forty they look as well as the latter at twenty. They go half naked, and wear only a piece of cloth wrapped round their loins, which hangs down to their knee. They are much addicted to ſtrong liquor, particularly arrac; and when they ſee an European veſſel, their firſt inquiry is after that beverage, for which and tobacco they give every thing they have, and ſometimes even their daughters and neareſt connections, to be kept for a certain time as ſervants, but not ſlaves. They are much more attached to the Dutch than to any other nation, and have the greateſt intercourſe with them. They are much afraid of other nations, and particularly the Malay pirates, becauſe the latter carry them away and ſell them as ſlaves.

On ſuch occaſions they defend themſelves with great bravery by means of ſlings, with which they hit a mark very accurately at a great diſtance. They can uſe alſo, with great dexterity, a ſort of javelins made of very hard wood, with which they can ſtrike fiſh at a conſiderable diſtance at the ſurface of the ſea. When theſe javelins are to be uſed againſt robbers, they harden the points of them in the fire, and dip them

them in lemon juice, by which means the wounds they inflict prove highly dangerous, and often mortal.

Some of the Salettians inhabit alſo ſeveral ſmall iſlands, not frequented by the other Malays, which lie in the ſtraits of Sinkapoor; but theſe are not ſo civilized as thoſe who live near Siak, and often while out fiſhing, or hunting ſwine, carry off many Portugueſe and other fiſhermen, whom they ſell as ſlaves.

They hunt the ſwine at ſea at different periods, when theſe animals, through natural inſtinct, emigrate as above mentioned. The Salettians, on theſe occaſions, ſmell the ſwine long before they ſee them, and make ready their boats. They then ſend out their dogs, which are trained to this kind of hunting, along the ſtrand, where, by their barking, they prevent the ſwine from getting on ſhore to conceal themſelves in the buſhes. During the paſſage, the boars go firſt, and are followed by the female ſwine and the young ones, all in regular rows, each reſting its ſnout on the rump of the one before it. Theſe animals, ſwimming in long rows cloſe to each other, form a very ſingular ſpectacle.

The Salettians, men and women, meet theſe ſwine in their ſmall flat boats. The former row, and throw large mats, made of the long leaves of the *pandamus odoratiſſima* interwoven through each other, before the leader of each row of ſwine, which ſtill continues to ſwim with great ſtrength; but, ſoon puſhing his feet into the mat, gets ſo entangled that he can no longer move them, or moves them only in a very ſlow manner. The reſt of the row, however, are neither alarmed nor diſconcerted, but keep cloſe to each other; and none of them leave the row, or the poſition in which they are placed. The Salettians then endeavour to row towards them in a lateral direction; and the buſineſs of piercing the ſwine is conſigned chiefly to the women, each of whom is armed with a long large javelin, in the form of a ſpontoon, headed with iron, and with which they ſtab as many of the ſwine as they can reach, always drawing back the weapon. Beſides this inſtrument, they have alſo a number of ſmaller javelins, about ſix feet in length, without iron points, which they carry in their arms, and which they throw to the diſtance of 30 or 40 paces, in ſuch a manner as to kill thoſe which they cannot reach with the long javelin. As it is impoſſible for them to throw mats before all the rows, or to kill all the ſwine in ſo ſhort a time, the reſt of theſe animals ſwim off in good order, and in regular rows, towards the place where they are taught by inſtinct that they ſhall find better nouriſhment, and for this time eſcape the danger

till

till they return to their uſual place of reſidence. As the ſwine which have been pierced float every where around, they are picked up and put into larger boats, which follow for that purpoſe.

If theſe hunters fall in with any veſſels belonging to the Chineſe, of whom a great many are ſettled at Malacca, and who on ſuch occaſions become ready purchaſers of their booty, they ſell to them their ſwine, and receive for thoſe that have an inch and a half thickneſs of lard on the breaſt at the rate of a piaſter a-piece. It may readily be believed that none of them are purchaſed by the Malays; for, being Mahometans, they abhor ſwine's fleſh, and, conſequently, theſe pagan ſwine-hunters; to whom they do all the miſchief they can, by deſtroying their huts, robbing them of their property, and often aſſaſſinating them, without ever being called to an account by any one for their depredations.

Of the ſwine which they cannot ſell, they firſt cut off the head; they then ſkin them and cut them up, chiefly for the ſake of the lard, but ſave as much of the fleſh as may be neceſſary to feed their dogs, and throw the reſt into the ſea. They then row their boats, laden with lard, to a ſolitary place on the coaſt unfrequented by the Mahometan Malays, where they melt the lard, and preſerve the greaſe in large earthen veſſels, or *bojans*, which are manufactured in Siam. This greaſe they ſell to the Maki Chineſe; and it is uſed not only by the common people inſtead of butter, as long as it is not rancid, which is ſeldom the caſe in Malacca, but alſo for burning in their lamps inſtead of coco-nut oil.

IX. *Inquiries into Coloured Light, by a Collation of the Experiments and Obſervations made by Sir Iſaac Newton on that Subject; together with ſome additional ones. By Governor* POWNALL *.

To Mr. Tilloch.

SIR,

I SEND you my paper on *coloured light*; an hypothetic theorem inſtituted to prove, not aſſuming to have proved, that there is but one *primary colour* in our ſolar ſyſtem, and that all the reſt in the priſmatic image are, on one ſide of the ſcale, only gradations of that colour towards pure light; and, on the other ſide the ſcale, merely degradations from light towards the actual abſence of it; and that green exiſts only as an intermediate compound between the two.

* Communicated by the Author.

The

The proceſs of this theorem is founded on the experiments actually made by Sir Iſaac Newton, and on ſome further experiments and reaſoning, which his quæries ſuggeſt.

I wiſh this imperfect attempt may excite ſome real philoſophers to take up this point of inquiry.

IT appearing from Dr. Herſchel's obſervations, publiſhed in the Philoſophical Tranſactions 1796, that the ſolar light does not proceed from the ſun as an igneous body, but apparently from inflammable and inflamed vapours floating on the ſurface of its atmoſphere; and it having been long known that this white lucid light could be decompoſed into a number of coloured rays, each homogeneous, as has been ſuppoſed; each "originally endued," as Sir Iſaac Newton ſays, "with its reſpective colour;" or, as he otherwiſe expreſſes the ſame fact *, "with that which each is originally diſpoſed to exhibit;" it occurred to my mind, on reading this in the year 1797, that theſe obſervations of Dr. Herſchel, if finally found to be true in nature, and followed up by an induction of combining facts, might lead to a diſcovery of that property of light which hitherto appeared to me to be inexplicable, at leaſt unexplained, viz. that every the minuteſt ray of which it conſiſts is a compound of ſeven, or three, principal primary homogeneous coloured rays, each originally endued with its reſpective colour, as conſtituent elements of this lucid white light.

It occurred to me, that if *ſimilar* coloured lights, ariſing from the aërial inflamed vapours, or flames of bodies combured in our terreſtrial atmoſphere, ſhould be found to obſerve the ſame refractions, and by theſe refractions, as they never are purely homogeneous, ſhould in their decompoſition obſerve and be affected by the ſame laws as operate on the ſolar light; the ſolar light may, by direct analogy, be ſuppoſed to be only a compound of inflamed vapours taking the reſpective colours of their flame, and being endued with their reſpective refractions, according to ſuch attractions as the reſpective ſubſtances from which they ariſe are liable to. Whilſt I conſidered this, I recollected that I had read in, and learned from, Sir Iſaac Newton, ſpeaking of flame, according to an expreſſion which he uſed at that time, "as ſmoke red hot," that, "according to the nature of the ſmoke, the flame is of

* There are ſome phænomena in the different colours of the fixt ſtars which ſeem to indicate that the coloured light of their ſyſtem may differ from that of the ſolar one.

various

various colours, as that of ſulphur, blue; that of copper opened with ſublimate, green," &c.

To inveſtigate this analogy according as the various lights which come to our ſenſes preſent themſelves to us, I here directed my inquiry, 1ſt, To learn what colours the various inflamed vapours of terreſtrial bodies combured in our atmoſphere throw out: 2dly, Whether theſe colours were homogeneous; and if not, 3dly, How far they obſerved ſuch ſimilar refractions in their decompoſition as the ſolar light doth. From the ground of a ſpeculative opinion, which I have long entertained in my own mind, that, although the white lucid light of the ſun is only a compound, yet light as light exiſts in a lucid, homogeneous, elementary, white and uncompounded ſtate, and only becomes otherwiſe by mixing or paſſing through oxygenous and other airs, or ſuch vapours as reſult from combuſtion, and, by being ſuffuſed with the colours furniſhed by theſe, becomes leſs and leſs pure, and, conſequently, tainted with colours. I applied my obſervations firſt to the electric ſpark. From the inſtantaneous and tranſient nature of this light, the decompoſition could not be examined: it is known, however, that electric light paſſed through an exhauſted receiver exhibits tremulous rays of various colours ſomewhat ſimilar to the *aurora borealis*; ſo that it comes out that this light is a compound. The next light which met my attention was that of the ſparks of iron deflagrated in vital air. Although I could not examine this by experiment for the ſame reaſon as before, yet I may venture to pronounce that, this being nothing but the white heat of fuſed iron, it muſt be a compound light. The next was the very brilliant light of phoſphorus burnt in vital air, ſo blazingly bright, that no eye can ſtand the firſt ſtroke of it. The next was the lucid light of camphor inflamed by ſpirit of wine, whoſe blue may be ſuppoſed, in ſome degree, to tinge the brilliancy of the white: theſe decompoſed into a faint ſpectrum of the principal colours. The next was the light of ſpermaceti burnt in Argand's lamp, wherein the ſmoke is in great meaſure conſumed. This was ſaid by Dr. Franklin to come the neareſt to day-light of any other terreſtrial light. This decompoſes into red and yellow, ſomewhat greeniſh as it runs into the blue. The flame of wax was the next light: this has a ſuffuſion of yellow, and decompoſes into a red and greeniſh yellow and blue. The flame of tallow was the next, which Sir Iſaac Newton calls yellow: this decompoſed into a red, a blueiſh green, and blue.

From the ground of a reaſon which will be given below,

I began

I began in examining coloured lights by that of red-hot iron *. This viewed through a prism decomposed into a deeply tinged red, and a deep blueish green only; the gradations of blue, if they are not absorbed in the deep green, are so rapidly absorbed in darkness as to be hardly if at all visible.

I had read in Lavoisier of a red light, but was not able to ascertain the fact: some time after, I met with a passage in a translated edition of Lavoisier, inserted by the editor, wherein he mentions an example of a red light produced by the vapour of strontian inflamed by spirits of wine, and represents it of a deep blood colour. In trials which I had an opportunity of seeing, and of making myself, this light, which, mixed with the inflamed vapour, appeared to my eye to give out a light by intermitting flashes tinged with rather a carmine red; and I learn that nitrate of strontian gives to the flame of spirit of wine a bright carmine red. Dr. Gibbes † showed me another instance of muriate of strontian giving a suffusion of red to the flame of spirit of wine. Nitrate of *lime*, formed into the salt called Baldwin's phosphorus, does the same.

I am taught that nitre, distilled with half its weight of sulphur, gives a *yellow* ‡ acid liquor yielding *red fumes*.

Hydrogen and carbon, united with oxygen in the constituent parts of animals and with azote in vegetables, form oil and fat: the flame of these is yellow or yellowish, as the flame of sulphuric and bituminous bodies is blue suffused with green; the yellow of the former runs into reddish fumes, the blue of the latter into greenish fumes. The oleaginous parts of bitumens being yellow, explains the reason why their blue flame is disposed to green. Verdegris inflamed with spirit of wine gives out a green flame; but this decomposes into a red and blue, with an intermediate blue-green.

Although these coloured flames and vapours are not homogeneous, yet they observe, according to various attractions which the aëriform substances, their respective bases, are liable to, the same laws of refraction and decomposition as the solar light doth, the primary colours of which are not themselves homogeneous.

* Although there is no decided flame in this case, yet whoever views a piece of red-hot iron along its sides will see a waving tremulous vapour.

† I beg here to acknowledge the obligations I received from Dr. Gibbes, in the arrangements of any matters connected with chemistry which I had occasion to refer to.

‡ It may be worth noticing, by the by, that as this yellow liquor gives, in its aëriform state, a red fume; so sulphur, yellow in its solid form, gives out, in its aëriform state, a blue flame.

As

As ſubſtances acidified by oxygen "*differ* from each other *only* according to the ſeveral natures of the oxygenated or acidified baſes," why may not this be the caſe of coloured flame or vapour? Their colour *differs only* according to the nature of the baſes thus illuminated by their combination with light.

In examining and obſerving upon theſe facts *, both by priſms and lenſes, in this comparative view, it appeared theſe terreſtrial coloured flames and aëriform vapours combured in our atmoſpheric air, obſerve the ſame laws of refraction and decompoſition as the ſolar light in every inſtance which can be brought under experiment. And that, as the ſolar light proceeds from inflamed vapour on the ſurface of the ſun's atmoſphere, as Dr. Herſchel ſtates, this ſolar light muſt ariſe from vapours having ſimilar baſes as theſe terreſtrial coloured lights have, allowing for an apparent different purity; and muſt, having the ſame properties, be of the ſame nature. So that, although the ſolar light, coming to our ſight in its compound ſtate, is a bright lucid light; yet, being decompoſed by the different refractions, which the ſeveral ſubſtances or baſes from which it proceeds are liable to, it is reſolved, exactly as the terreſtrial lights above examined are, into the conſtituent coloured lights which the ſeveral baſes give out.

I will not venture, on the ground of the obſervations ſtated, and on the reaſoning attached to them as above, to lay it down as a propoſition abſolutely proved; but will hazard † it as a quære, open to further inveſtigation.

May not the ſun, an unignited body, and ſimilar to our terreſtrial globe in all its external appearances, (but poſſibly of variety in its mineralogy,) produce an atmoſphere of more

* It is hardly worth while to deſcribe the method I obſerved to do this, yet it may not be amiſs. I had prepared a tall long box, painted black on the inſide, with a ſliding door in the front, in which was bored a hole about half an inch diameter: the uſe of this ſliding door was to raiſe or lower the hole to the height of the coloured light which I had occaſion to try. There was alſo a door on the ſide by which to put in the lights, and a valve at the top to let out the ſmoke when I ſhould obſerve that it affected the light. By applying a priſm to this hole, I obtained from all the lights, except the blue, a priſmatic ſpectrum: the light of the blue was ſo weak, that I could not obtain a ſpectrum from it coming through the hole; I therefore drew up the ſliding-door entirely, and ſo turning the priſm as to direct the refracted rays down upon the ground near under the table on which the box ſtood, and into the dark, obtained, yet imperfectly, the ſpectrum above noticed. By lenſes applied to the hole, the differences of the different focuſes were as the refractions inverſely.

† This I ſtated to Dr. Herſchel by letter, dated March 17, 1797; and had afterward a converſation with him at Slough on this ſubject of the coloured rays of the ſolar light.

ſublimed

ſublimed and purer vapours than our earth does; and ſo exhibit purer and more homogeneous coloured lights than the terreſtrial inflamed vapours do, although in every other reſpect of a ſimilar nature? May not this explain the reaſon why ſome of the fixed ſtars, alſo ſuns, appear of a green or red colour? for, if their bodies conſiſt of a different mineralogy, and the vapours of their atmoſpheres be thus diſpoſed to throw out different, as for inſtance green and red lights, in a greater proportion than the other colours, they will thus, of courſe, have this or that appearance of being green or red; which is a known phænomenon. Nay, even the white lucid light of the ſun, apart all conſideration of circumſtances which may affect it as it comes through our atmoſphere, gives out at times a ſomewhat differently ſuffuſed white, according as this or that conſtituent of its compound light may be in a greater or leſs proportion.

May not the moon, producing an atmoſphere viſible where no light of the ſun can reach, and therefore not a reflected light, poſſeſs, as a planet, an atmoſphere very different from what thoſe ſuns produce, perhaps of a phoſphoreſcent nature, which exiſts with little heat? And may not the viſible atmoſphere of Venus, conſiſting of enlightened vapours, be of the ſame ſort? I will ſtrengthen the propoſition of this quære from the opinion of Dr. Herſchel on this planet: "It cannot ſhine (he ſays) by a borrowed light, ſo that this faint illumination muſt denote ſome phoſphoric quality in the atmoſphere of Venus."

Thus far I proceed in my reſearches on the ground of the ſolar light being a compound of various rays, ſuppoſed to be originally diſpoſed to exhibit ſeven or three primary colours; which colours are alſo ſuppoſed to be homogeneous. A doubt, however, has always occurred to me, firſt, whether this opinion of the homogeneity of the ſeven primary colours be founded in fact: and, in the courſe of examining this firſt doubt, a further doubt ariſes, whether there are *ſeven* or only *three* principal colours, as Sir Iſaac Newton expreſſes it; or whether there be more than *two*, and perhaps not more than *one*. Stating then what follows as a doubt followed up by inveſtigation, it appears to me, that the ſpace which each priſmatic primary colour (as it is called) takes in the priſmatic ſpectrum, has not one only angle of refraction within its extent, but a ſucceſſive ſeries of angles of refraction, and a ſucceſſive ſeries * of innumerable circular or orbicular images of the ſun (as Sir Iſaac Newton ſtates the fact), ſo cloſely inter-

* Optics, experiment 5, p. 32. *Vide* fig. 15. plate 3. of book i. part i.

fecting

fecting each other, that the point of interfection is hardly perceptible to a common eye unaffifted, if at all. Although, therefore, the centres of thefe circles may approach to each other at an infinitely fmall diftance; yet they are placed under different refractions, and muft, of courfe, give a fucceeding tint more refracted than the preceding: and as thus throughout the fpace of the commonly called homogeneous colour, fo muft the indifcriminate feries of refractions, and of the interfecting images of the fun, proceed *gradatim*, and not *per faltum*, into the next fpace of the next primary colour; and fo through that into the next; and in like manner throughout the whole. The angle of refraction at the point where the fpace in the fpectrum, which contains the *red*, is fuppofed to end, exceeds that which is on the outfide of the fame fpace, where it commences, in a much greater degree than it doth the next contiguous and continuous feries of angles of refraction of the fpace which contains the *orange*. And the fame holds good of the laft angle of refraction at the point where the orange ends, or is fuppofed to end, and where the yellow commences, or is fuppofed to commence; and fo through all the gradations of colours. Sir Ifaac Newton, in the hypothefis of his theorem inftituted for experiment, fo ftates it. As therefore the angles of refraction at the centres of the innumerable * circles or orbicular images of the fun in the prifmatic fpectrum, as ftated by Sir Ifaac Newton, which form any one of the principal colours, homogeneous as they have been called, thus differ in their degrees, fo muft the tints in each fuch colour differ in a feries of gradations; and cannot therefore, however they may to a tranfient or undifcriminating view appear, be abfolutely homogeneous: "for, the light which I here call homogeneous (fays Sir Ifaac Newton) being not homogeneal, there ought to arife fome little change of colours from its heterogeneity †." The *red*, by fuch a fucceffive feries of tints, is heightened towards and expanded into the *orange*; the orange, by a like fucceffion of tints, into the yellow; and the yellow, in like manner, into the white.

As thefe coloured lights on this end of the fpectrum heighten by gradations of tints more and more illumined, as if purging themfelves from the fuffufion of colour into pure light: fo the blues at the other end of the fpectrum, by a like gradation of tints, or rather hues, deeper and deeper, weaker and weaker, go off into darknefs, in "a continued feries, according to their degree of refrangibility ‡."

* *Vide* fig. 15. of plate 3. book i. part i. Newton's Optics.

† Newton's Optics, book i. part xii. prop. 11. p. 107.

‡ Newton's Optics, p. 32.

Having thus stated the *idea* of the successive gradations of the prismatic colours, contrary to what is commonly supposed, of each being homogeneous; let us now refer to the *fact*, as it actually exists. But before we examine the two gradations here stated of the red and blue (if blue be a colour), we will examine the *intermediate green*.

[To be continued.]

X. *On a new Combination discovered in Zaffer, which Brugnatelli considered as the Cobaltic Acid. By* C. DARRACQ *.

BRUGNATELLI, in a paper published in the *Annales de Chimie* for the month of Pluviose, year 8, mentions several experiments on zaffer, or the gray oxide of cobalt, in which he thought he had discovered a new acid. I shall give an account of some of these experiments, and also of the properties ascribed to his cobaltic acid.

Having suffered zaffer to remain some time with ammonia, after a few days digestion in the sun, he obtained a red liquor known under the name of *ammoniure of cobalt*, which he filtered, and evaporated to dryness. The concrete residuum obtained appeared to him to be composed of two distinct substances; one of a dark red, and the other of a pale yellowish colour. The red part dissolved in water, and the yellowish remained in the filter. This residuum he found to be pure oxide of cobalt. The part soluble in water was evaporated, and deposited, on cooling, some small crystals, which Brugnatelli found to be a combination of the new cobaltic acid with ammonia. The supernatant liquor still possessed an evident character of acidity.

The author observes, that his acid could be obtained coloured or without colour, according to the means employed to obtain it. When the evaporations are effected by means of fire, they leave a residuum, which, when dissolved in water, gives cobaltic acid almost colourless; while those effected in the sun give it always more or less red. Brugnatelli, desirous to ascertain whether his acid was formed during the operation of which I here speak, or whether it existed quite formed in the zaffer, boiled for twenty-four hours six pounds of this substance in eight pounds of water. He filtered the liquor while warm, and then caused it to evaporate. When

* From the *Annales de Chimie*, No. 121.

reduced to one-half it became turbid: the evaporation being continued till there remained only one-third of the liquid, it was taken from the fire, and it then depoſited a white matter, which was collected on the filter. The liquor which paſſed through had a yellow colour and a taſte ſenſibly acid, and exhibited the ſame phænomena in every thing as the cobaltic acid obtained by the proceſs before indicated.

The following are ſome of its properties, which Brugnatelli conſiders as characteriſtic:

1ſt, It precipitates a ſolution of ſilver.

2d, It precipitates lime water in a white coagulum, inſoluble in water and in exceſs of acid.

3d, It can be ſeparated by alcohol from its aqueous ſolution.

4th, It precipitates acetite and muriate of barytes.

I ſhall now deſcribe the experiments I made on this ſubject, and I ſubmit the conſequences I have deduced from them to the examination of the Inſtitute.

Exp. I. I formed ammoniure of cobalt by ſuffering ammonia to macerate, in the ſun, over zaffer. I took care to ſtir the mixture, which was contained in a matraſs: it ſoon aſſumed a red colour, and, forty-eight hours after, a bright red colour. I remarked in this liquid a pretty abundant cryſtallization under the form of white and brilliant needles. This cryſtallization was permanent until the ſun had again heated the liquid. It then diſappeared, and diſſolved in the liquor. In this ſtate the ammoniure of cobalt was filtered, and put into a retort: as it was heated it aſſumed a violet red colour, which became darker till it aſſumed a beautiful red wine colour. When the greater part of the ammonia was evaporated the mixture acquired a greeniſh colour, and, by reſt, there was precipitated a matter of the ſame colour as the ſolution. The liquid being then filtered while warm, and evaporated to dryneſs, there were obtained the rudiments of cryſtals, the form of which could not be determined. White and brilliant particles were remarked. The reſt of the matter had a yellowiſh colour. I poured water over this reſiduum, ſtirring it with a platina ſpatula. The part leaſt coloured diſſolved entirely, and communicated to the water a ſtraw colour. This liquor was acid, and poſſeſſed ſome of the properties announced by Brugnatelli. I ſhall deſcribe hereafter the experiments to which I ſubjected it, and the reſults of which, carefully examined, prove that it was not an acid formed by cobalt. The two reſiduums obtained in this experiment, one of a green and the other of a yellowiſh colour, were found not to be pure oxide of cobalt, as Brugnatelli ſays, but a combination of that oxide with arſenic acid.

Exp.

Exp. II. Having prepared a new quantity of ammoniure of cobalt, as in the first experiment, I subjected it to spontaneous evaporation in a place where the sun accelerated the volatilization of the ammonia. In proportion as the liquid evaporated, there was precipitated a flaky matter of a whitish rose colour, which sensibly increased till the ammonia was evaporated. The liquor emitted no odour, but it had retained a sufficiently beautiful rose colour. The liquor was again evaporated to dryness, and the remaining matter was dissolved in distilled water. This solution, of a light rose colour, was acid, and possessed the properties of the acid obtained in the first experiment.

The residuum left on the filter was carefully examined, and found to be arseniate of cobalt.

Exp. III. I took, as the author of the memoir did himself, a kilogramme of zaffer, which was reduced to an impalpable powder. I boiled it, for half an hour, in three litres of distilled water. The liquor, which I filtered while warm, had a light colour and a sensible savour; and, being evaporated in a porcelain capsule, the liquid, towards the end of the operation, became turbid. I then continued till there remained only about a hectogramme of the liquor. By cooling I obtained a crystallization in needles. Having filtered the mixtures, stirring them, the crystals remained on the filter, and the supernatant liquor passed through, very transparent, of a bright yellow colour. Brugnatelli, in making this experiment, did not remark the crystallization; he only observed a white matter, which assumed a rose colour by coming into contact with the air, and which he found to be oxide of cobalt. The needle-formed crystallization which I obtained produced, as soon as it was heated, a considerable disengagement of arsenic. When carefully examined it was found to be arseniate of cobalt, and not pure oxide, as had been announced. The fact is of little importance, no doubt, in regard to the discovery of Brugnatelli; but it may serve to support my opinion, if the results of my experiments were not more than sufficient to prove that the cobaltic acid does not exist.

The liquors of the first, second, and third experiments, found to be acid, and of the same nature, were subjected to the following proofs, and comparatively with the arsenic acid:

1st, This liquor was precipitated of a yellow colour, like that of orpiment or sulphuret of arsenic, by sulphurated hydrogen and alkaline hydrosulphurets. This precipitate was indeed sulphuret of arsenic, and not sulphur precipitated, as believed by Brugnatelli.

2d, It precipitates ammoniure of copper of a blueish green.

This property is that of the arsenic acid. This combination is known under the name of *arseniate of copper*.

3d, Sulphate of copper mixed with this liquor gives a precipitate of the same colour as the ammoniure of that metal. The arsenious acid possesses this property also, though not in so high a degree as the pretended cobaltic acid; but a similar precipitate may be obtained by making use of artificial arseniate of cobalt. The results then are the same: no difference can be perceived between them.

4th, It precipitates nitrate of silver white. The arsenic acid possesses this property.

5th, It precipitates nitrate of mercury of a straw colour. It is well known that the arsenic acid exhibits the same phænomenon.

6th, It precipitates lime water in a white coagulum insoluble in water, but not insoluble in excess of acid, as Brugnatelli says; since it, indeed, redissolves with the same facility in the pretended cobaltic acid as in the arsenic acid. The error of Brugnatelli, in my opinion, arose from his not having employed a sufficient quantity of acid.

7th, It precipitates acetite and muriate of barytes. I assured myself that this precipitation arose from some atoms of sulphuric acid contained in the acid called cobaltic. The arseniate of cobalt also disguises its solutions.

8th, With tincture of galls newly made it forms an abundant yellowish precipitate. It is well known that the arsenic acid produces the same phænomenon.

9th, This last experiment seems to have been considered by Brugnatelli as the most characteristic of his cobaltic acid: he observed that alcohol separated his solution from its aqueous solution, and the arsenic acid, dissolved in alcohol, possessed the same property: by these processes he obtained his concrete acid. This property of the cobaltic acid at first astonished me; but, presuming that I might find an explanation of it by examining the acid thus precipitated, I subjected it to the following experiments:

I. When heated on charcoal at the flame of the blowpipe it emitted white vapours, which were evidently arsenic.

II. A small portion, heated with borax, communicated to it a violet colour.

III. This acid, being precipitated only by alcohol, I examined the liquor after having separated it by the filter. Sulphurated hydrogen produced in it an abundant yellow precipitate, found to be sulphuret of arsenic.

IV. The acid, when separated, was scarcely soluble in water.

V. When

V. When mixed with ſome drops of arſenic acid, it diſſolved completely, and reappeared with its firſt properties.

After theſe trials I concluded that the acid of Brugnatelli could be only arſeniate of cobalt diſſolved in exceſs of acid.

To aſcertain this, I diſſolved pure oxide of cobalt in arſenic acid. I evaporated the ſolution to dryneſs, and added to the reſiduum diſtilled water. After ſtirring the mixture, it was filtered. The liquid obtained had a light roſe colour, and poſſeſſed all the properties of the cobaltic acid.

This liquor, mixed with alcohol, was abundantly precipitated: this precipitate, when collected, exhibited all the phænomena obſerved in concrete cobaltic acid.

It appears then certain, from the reſults here given, that no cobaltic acid exiſts: that the combination of the arſenic acid and the oxide of cobalt led Brugnatelli into an error, ſince it is this combination with exceſs of acid that is extracted from zaffer by the means deſcribed in that memoir.

XI. *Method of making Lime cryſtallize.* By Trommsdorff*.

THE cryſtallizable quality of lime was firſt diſcovered by Schaub. Bucholz obtained alſo very beautiful cryſtals of this earth by boiling it with its muriate. I have verified this diſcovery, and have found that cryſtallized lime may be obtained as well in winter as in ſummer, only that the ſalt prepared during the former ſeaſon is in larger and longer cryſtals. To obtain theſe cryſtals, boil any quantity, at pleaſure, of muriate of lime with a fourth part, or leſs, of cauſtic lime, and concentrate it, if in winter, till a drop of the ley, made to fall on a cold ſtone, aſſumes the conſiſtence of ſyrup without cryſtallizing or becoming fixed. The matter is then to be filtered through a piece of cloſe cloth extended above an earthen or porcelain capſule, which muſt be covered after filtration by a ſimilar capſule or a wooden lid, in order that the matter may cool as ſlowly as poſſible. In this manner may be obtained very long but very fine cryſtals of lime, which it is neceſſary to waſh in alcohol in order to free them from the adhering muriate. This operation cannot be undertaken with leſs than ſome pounds of the muriate of lime. It is well known that, when muriate of ammonia is diſtilled with exceſs of lime to decompoſition, a part of the reſiduum adheres ſo ſtrongly to the veſſel that it is almoſt

* From *Journal de Chimie*, by J. B. Van Mons, No. 2.

impossible to detach it by softening it. This hard mass is formed, in a great measure, by the lime crystallized in a confused manner; which it is more difficult to dilute in the water than the same earth in powder, or very much divided.

XII. *Result of the Observations of the new Star, discovered on the 1st of January* 1801, *at the Royal Observatory of Palermo. By* Joseph Piazzi, *Director of the Observatory* *.

1. HAVING been engaged for nine years in verifying the positions of the stars as collected in the catalogues of various astronomers, I was searching, on the 1st of January 1801, among many others, for the 87th in the catalogue of the zodiacal stars of the abbé De la Caille, when I observed that this star was preceded by another, which, according to my usual custom, I wished to observe also, especially as it did not interrupt the principal observation. Its light was somewhat faint, and in colour resembled that of Jupiter, but like that of many others which in regard to their magnitude are usually placed in the eighth class. At that time no doubt arose in regard to the nature of it; but on the evening of the 2d, having repeated my observations, and finding that they did not correspond either in time or zenith distance, I suspected that some error had been committed in my observations on the preceding day. I then began to entertain some idea that it might perhaps be a new star. In the evening of the 3d my conjecture was confirmed, as I assured myself that it was not a fixed star. However, before I would speak of it, I waited till the evening of the 4th, on which I had the satisfaction of finding that it had moved according to the same laws which it had observed on the preceding days. From the 4th to the 10th the sky was overcast: on the evening of the 10th it appeared to me in the telescope, accompanied by four others nearly of the same magnitude. Being still uncertain in regard to the nature of the new star, I observed them all as well as possible, and, comparing these observations with others which I made on the evening of the 11th, I easily distinguished the new one from the rest by its motion. I was, however, exceedingly anxious to see it when not on the meridian, and therefore employed myself

* From *Risultati delle Osservazioni della nuova Stella scoperta il de' 1 Gennajo all' Osservatorio Reale di Palermo da Giuseppe Piazzi, Ch. Reg. Direttore del medesimo.*

in

in examining and contemplating it with great care; but, notwithſtanding all my exertions, and thoſe made by my aſſiſtants Cacciatore and Carioti, both poſſeſſing excellent ſight, and accuſtomed to viewing the heavens with a night teleſcope and an achromatic of four inches aperture, it was not poſſible to diſtinguiſh it from many others by which it was ſurrounded. I was therefore obliged to be contented with ſeeing it on the meridian for the ſhort ſpace of about two minutes, that is, during the time it employed to paſs over the field of the teleſcope; the other obſervations, made at the ſame time, not permitting me to remove the inſtrument from its poſition. To render theſe obſervations, however, more certain, while it was obſerved by myſelf with a circle, it was obſerved at the ſame time by Carioti with a tranſit inſtrument. In this manner, the heavens being conſtantly nebulous, and often obſcured by thick clouds, our obſervations were interrupted till the 11th of February; after which the ſtar having approached very near to the ſun, it was no longer poſſible to ſee it at the time of its paſſing the meridian. I reſolved then to ſearch for it beyond the meridian by the means of azimuths; but having fallen ill on the 13th of February, I was not able to make any further obſervations. Thoſe, however, which have been made, though not at the neceſſary diſtance to aſcertain the real path which this ſtar purſues in the heavens, are ſufficient, as far as I am able to judge, to make known, with certainty, the nature of it; as may be ſeen by the reſults I have deduced from them.

The magnifying power of the teleſcope of the circle was 50, and that of the teleſcope of the tranſit inſtrument 80; whence it was judged by Carioti that the new ſtar is of the 7th or 8th magnitude.

II. Combining in a parabola the two obſervations of the 1ſt and 19th of January with the third of the 11th of February, which were very good, I find the following reſults:

Perihelion	-	-	-	$4^s\ 5^\circ\ 28'\ 36''$
Long. ☊	-	-	-	2 19 43 0
Inclination	-	-	-	10 34 0
Log. diſt. of the perihelion			-	0·3713077
Paſſage of the perihelion July 1801			-	3·6985

As theſe elements, however, did not agree with other obſervations, I tried another parabola combining other obſervations, and experienced the ſame difficulties. Reflecting afterwards, that to make the two obſervations, that of the 1ſt of January and that of the 11th of February, agree, it was neceſſary to ſuppoſe 0·26 of difference between the radius vector correſponding to the firſt, and the other correſponding to the

second, I described mechanically my first parabola; and applying to it the longitudes and latitudes thus calculated and observed, it was easy for me to ascertain that the motion of this star could not, indeed, be represented in the arc of a parabola such as is nearly described by comets.

III. From parabolic hypotheses I proceeded to circular, and, having made a few suppositions, I found two radii, viz. 2·7067 and 2·6862, with each of which the observations could be represented much better than with any parabola; for, as the planets describe ellipses more or less eccentric, and not circles, it is to be supposed that our planet will not deviate from this law. I ought therefore to have continued my calculations on an ellipsis; but as the observed arc was very small, the results would have been uncertain, and the labour tedious and painful. On this account I preferred the circle, especially as the elements obtained from the ellipse to determine the place of this star do not appear to me to be more certain than those afforded by the circle.

IV. On the 10th of January the star, from being retrograde, became direct. Setting out, therefore, from the observations of this day, I endeavoured to find its elongation while stationary, which was $4^s\ 4°$; whence we have for mean radius of its orbit 2·9352. But the difference between this radius and the other, obtained by the motion of the star between the 1st of January and the 11th of February, seems to indicate a great eccentricity, whereas the series of the observations seems rather to indicate that the eccentricity ought to be small: on the other hand, the diameter, deduced from the elongation of a planet while stationary, can never be very exact, and particularly in the present case.

V. During the first observations, bringing the star under the horizontal thread of the telescope, it remained, as it were, entirely covered; and, as the thread subtends to the eye an angle of about 6″, I judged the diameter of the star to be somewhat greater, that is to say, 7″. During the last observations I was not able to form any judgment of its diameter, on account of the darkened state of the atmosphere.

VI. RESULTS.

Radius of the orbit				2·6862
Motion in the orbit from January 1st to February 11th		9°	2′	29·7″
Epoch 1801	2^s	8	46	22
Motion in 100 days	0	22	6	33·7
Longitude ☊	2	20	46	48
Inclination of the orbit		10	51	12

Mean

Mean distance deduced from the time of being stationary - - 2·9352
Tropical revolution, deduced from the mean distance according to the law of Kepler - - years 5·03
Sidereal revolution from the motion in the orbit - - days 1628·27
Apparent diameter at the distance of the earth from the sun - - 19″
Magnitude, 1⅓d that of our earth.
Opposition, about the 1st of March 1802.

VII. Table of the Mean Time, Right Ascension, and Declination of the new Star as observed; together with the Longitude of the Sun, and the Logarithm of its Distance from the Earth.

	Days of the Month.	Ten Thousandths of the Day in mean Time.	Right Ascension.	Declination.	Sun's Place.	Log. Distance of ⊕ from ⊙
January.	1	3635	51° 47′ 48·7″	15° 37′ 43·5″ N	9ˢ 11° 1′ 33·1″	9,992617
	2	3606	43 27·7:	41 5·5	12 2 31·7	9,992629
	3	3577	39 36::	44 31·6	13 3 30·2	9,992641
	4	3547	35 47·2	47 57·6	14 4 29·0	9,992652
	10	3378	23 1·5:	16 10 32·0	20 10 29·5	9,992768
	11	3350	22 26·0	14 30, est.	21 11 29·5	9,992794
	13	3295	22 34·5	22 49·5	23 14 28·0	9,992848
	14	3268	22 55·8	27 5·7	24 14 27·3	9,992882
	17	——	27 35::	40 13·0	——	——
	18	——	28 45::	——	——	——
	19	3136	32 2·2	49 16·1	29 19 14·1	9,993060
	21	3084	38 34·0	58 35·9	10 1 21 2·5	9,993151
	22	3059	42 21·3	17 3 18·5	2 21 55·1	9,993196
	23	3033	46 43·5	8 5·5	3 22 46·4	9,993242
	24	——	51 45::	——	——	——
	28	2909	52 13 38·3	32 54·1	8 26 45·5	9,993522
	30	2860	27 2·1	43 11·0	10 28 10·6	9,993645
	31	2837	34 18·8	48 21·5	11 28 55·5	9,993708
February.	1	2813	41 48·0:	53 36·5	12 29 36·6	9,993773
	2	2789	49 45·9	58 57·5	13 30 17·0	9,993851
	4	——	53 7 45::	——	——	——
	5	2719	15 40·5	18 15 1·0	16 32 13·9	9,994083
	8	2650	44 37·5	31 23·2	19 35 2·2	9,994328
	11	2583	54 16 23·1	47 58·8 N	22 35 41·3	9,994588

N. B. The observations marked with two points (:) are a little doubtful; those marked with (::) very uncertain.

VIII.

VIII. Table of the Geocentric Longitudes and Latitudes of the new Star, both by Observation and Calculation; together with their Differences.

	Days of the Month.	Geocentric Longitude. Observat.	Geocentric Longitude. Calculat.	Differ.	Geocentric Latitude. Observat.	Geocentric Latitude. Calculat.	Differ.
January.	1	1s 23° 22′ 58·5″	1s 23° 21′ 59·2″	−59·3″	3° 6′ 32·4″	3° 6′ 50·2″	+17·8″
	2	19 44·8	18 40·2	−64·6	2 13·1	2 27·7	+14·6
	3	16 49·3	15 47·1	−62·2	2 57 58·9	2 58 6·3	+ 7·4
	4	14 16·5	13 18·9	−57·6	53 44·5	53 48·4	+ 3·9
	10	7 59·4	7 19·5	−39·9	28 50·9	28 31·8	−19·1
	11	8 25·7	7 43·4	−42·3			
	13	9 58·0	9 38·9	−19·1	16 49·0	16 21·0	−28·0
	14	12 1·6	11 32·6	−29·0	12 47·1	12 23·2	−23·9
	19	25 49·4	25 51·5	+ 2·1	1 53 28·3	53 1·3	−27·0
	21	34 21·8	34 23·4	+ 1·6	45 58·9	45 31·6	−27·3
	22	39 1·8	39 6·7	+ 4·9	42 18·7	41 51·3	−27·4
	23	44 15·6	44 17·4	+ 1·8	38 39·2	38 12·3	−26·9
	28	24 15 16·0	24 15 28·1	+12·1	20 58·7	20 32·0	−26·7
	30	30 5·4	30 23·0	+17·6	14 5·3	13 43·5	−21·8
	31	38 8·6	38 20·5	+11·9	10 45·0	10 22·4	−22·6
February.	1	46 19·6	46 38·0	+18·4	7 23·8	7 3·6	−20·2
	2	54 55·6	55 12·8	+17·2	4 0·7	3 47·0	−13·7
	5	25 22 43·5	25 22 52·8	+ 9·3	0 54 19·0	0 54 9·6	− 9·3
	8	53 17·9	53 15·6	− 2·3	44 42·7	44 50·9	+ 8·2
	11	26 26 26·1	26 26 24·8	− 1·3	35 47·9	35 50·4	+ 2·5

IX. The correspondence of the observed longitudes of this star with those calculated according to the circular hypothesis; its motion in the zodiac, from which it deviates only a little in its greatest latitudes; and its position between Mars and Jupiter; seem to leave no doubt that it is a real planet, and, in all probability, the very same deduced from calculation in the year 1772 by professor Bode, and announced to the Academy of Sciences of Berlin. That it was not then observed, though the zodiac was diligently examined by the best astronomers, ought to be ascribed chiefly, in my opinion, to its smallness compared with its distance from the earth and with its greatest latitudes; though it is not improbable that it was seen by the abbé De la Caille or Tobias Meyer. In the catalogues of the stars made by these two astronomers there are some observed only once; which I have not been able to find, though I searched for them repeatedly and at different times. If the original observations of Meyer are preserved at Göttingen, and those of the abbé De la Caille at Paris, they may possibly be yet discovered. In the end of my work on the position of the fixed stars, the printing of which, through the munificence of our gracious sovereign, is already far advanced, there will be given a catalogue of the lost stars, by which this research will be greatly facilitated.

X. It

X. It is the opinion of many aſtronomers, and I am inclined to agree with them, that there are ſeveral other planets of the like kind ſtill undiſcovered. But as aſtronomers either ſeldom obſerve ſtars beyond the 7th magnitude, or are ſatisfied with obſerving them once, or at moſt twice, there is reaſon to doubt whether theſe planets will be eaſily diſcovered. Had I not been accuſtomed to obſerve ſtars four, five, or ſix times, and even more, I ſhould certainly not have diſcovered the one in queſtion. Re-examining, as was the caſe, after a long time, the obſervations of the 1ſt and 2d of January, and finding that they did not agree, I ſhould have ſearched in the ſame place of the heavens for this new ſtar, and, not finding it, ſhould have claſſed it among the number of the doubtful ones; a thing I have been too often obliged to do with others, the obſervations of which I was prevented from continuing by the inclemency of the weather.

XI. Oriani, Bode, and Von Zach, had ſcarcely ſeen the obſervations of the 1ſt and 23d of January, which I communicated to them on the 24th, adding at the ſame time that on the 10th the ſtar from being retrograde had become direct, when they were of opinion that it was a new planet; and they next concluded that its elements were the ſame as thoſe which I had conjectured. But as the ſtar after the 23d began to decreaſe ſenſibly in magnitude and in light, being uncertain whether this ought to be aſcribed to its receding rapidly from the earth, or rather to the ſtate of the atmoſphere, which after that period became more overcaſt and obſcure; I began to entertain ſome doubts reſpecting its nature, and to believe that, in all probability, it was a comet and not a planet. It was only by calculating all the obſervations that my doubts were at length diſſipated; but, on account of other avocations and the bad ſtate of my health, I was not then able to pay proper attention to them. Finding myſelf ſomewhat better in April, I reſolved to calculate my obſervations; but having brought on a ſecond indiſpoſition while fixing the meridian of the metropolitan church, and being reduced to a weaker ſtate than before; being uncertain at what time I ſhould be able to reſume my ſtudies, and anxious to communicate my obſervations to the above-mentioned meritorious aſtronomers, I tranſmitted them to Lalande at Paris, Oriani at Milan, and Bode at Berlin. As yet, however, I have received no anſwer but from Bode, who, on ſeeing my further obſervations, is more and more confirmed in his firſt opinion, and only ſeems a little ſurpriſed that, as I was much inclined in my firſt letter to Oriani to conſider my new ſtar as a planet,

I ſhould

I should afterwards have considered it rather to be a comet. But had I communicated to him also the circumstance respecting the diminution of its light, he would, perhaps, have entertained some doubts of the same kind.

XII. As this star has not been lately seen, some doubt will remain in regard to its nature; and it may be difficult to discover it again on account of the uncertainty of the elements of the orbit which it describes, and more particularly its very small size. At present it remains above our horizon the greater part of the night, being in the sign of Cancer; but it is at too considerable a distance for me to hope that I shall be able to distinguish it: and, on the other hand, I am destitute of the instruments necessary to search for it, with any certainty, out of the meridian. About the beginning of November it will be much easier to discover it; and as the first days of March, the time of its opposition, will be the most convenient period for observing it with advantage, I entertain great hope that it will not escape the research of astronomers.

XIII. However, whether this new planet be found again by myself or by any other astronomer, I shall be the more gratified, as, after the example of a Halley, a Hevelius, a Bode, and a Herschel, who have inscribed in the heavens the glorious names of a Charles II. a Poniatowski, a Frederic, and a George III. all illustrious patrons of astronomy, I think I have an equal, and perhaps stronger reason, for inscribing in indelible characters that of the august and magnanimous founder of this observatory, our sovereign Ferdinand, conjoined with that of the indigenous deity of this part of his kingdom, which he renders more lively and happy by his presence. I have therefore informed those astronomers who are my correspondents that I have given to this new star the name of *Ceres Ferdinandea*.

> Telluris patriæ ductura à Principe nomen
> Astra inter, Siculis fulsit ab axe Ceres.
>
> *Michael Angelus Monti*, Scol. Piar.

SUPPLEMENT.

This short memoir not being yet published, I shall here subjoin the substance of a letter which I received from my esteemed friend Oriani, dated Milan, July 25, 1801, in which he communicates to me the result of his calculations, accompanied with those of some other astronomers who have done me the honour to employ their talents on my observations.

Oriani, who calculated in a parabola, found

☊ - - - - -	2^s 21° 48′
Inclination - - - -	9. 33
	Perihelion

Perhielion - - -	$4^s\ 10^\circ\ 16'$
Distance of the perihelion - -	2·1045
Passage of the perihelion June 1801 -	21·07

Mr. ——— found also in a parabola,

☊ - - -	$2^s\ 20^\circ\ 50'$
Inclination - - -	9 41
Perihelion - - -	4 8 38 25″
Distance from the sun - -	2·21883
Passage of the perihelion June 1801	$30^d\ 19^h\ 1^m$

The difference of the three parabolas calculated by Oriani, Mr. ———, and myself, none of which represent all the observations, confirm still more that the motion of this star cannot indeed be represented in a parabolic arc, as I have said in my preceding memoir.

The same person who calculated the second parabola tried to make the observations agree with a circle. His elements are:

Radius - - -	2·74
Epoch 1801 - -	$2^s\ 8^\circ\ 16'\ 20''$
☊ - - -	2 20 15 0
Inclination - -	0 11 21 0
Sidereal revolution - -	years $4\frac{1}{2}$.

In this circle the errors in excess amount to 2′ 30″, and those in defect to as much.

Burckhardt calculated in an ellipse, and found:

☊ - - -	$2^s\ 20^\circ\ 58'\ 30''$
Inclination - -	0 10 47 0
Aphelion - -	2 8 59 37
Passage of the aphelion Jan. 1801 -	1·3328
Eccentricity - - -	0·0364
Log. of the greater semi-axis -	0·4106586
Sidereal revolution - -	years 4·13

In this ellipsis the longitudes and latitudes of five observations are represented pretty well, there being only the difference of a few seconds between calculation and observation.

The astronomer who calculated the parabola and circle above mentioned, and whose name I was not able to make out, in the few leaves printed in German with which Oriani favoured me, suspected that there might have been some error in the copy of my observations transmitted to him. This was really the case in the first; but I afterwards sent another corrected copy, as well as to Lalande, Oriani, and Bode, all corresponding exactly with that on which I founded my own calculations.

However, that my exertions may correspond with the interest generally taken in my discovery, whatever the result

may be, and to leave no doubt in regard to the obſervations, I have again examined them; and in ſome places which were rather uncertain, and which I employed in my firſt calculations, I ſubſtituted others. I have alſo made an allowance for the deviation of the inſtruments, and employed all thoſe precautions which are uſual when great preciſion is required. The reſult of all theſe has been only a ſmall difference in ſome of the right aſcenſions, which can have little or no influence on the ultimate reſults of the calculation, and for which reaſon I did not think great exactneſs neceſſary in the firſt reductions. According to this laſt rigorous examination, $1{\cdot}5''$ are to be deducted from the firſt four right aſcenſions, and $1{\cdot}5''$ to be added to thoſe of the 10th, 11th, 14th, 19th, 21ſt, 23d, 28th, 30th, and 31ſt of January, and the 1ſt of February; and $3''$ muſt be taken from thoſe of the 5th and 8th of February. With the tranſit inſtrument the obſervations were made, different times, with all the five threads; and theſe I always preferred to thoſe made with the circle. I uſed thoſe by the circle when I failed with the tranſit inſtrument; as on the firſt four days; and on the 13th of January, when the obſervations were not made with all the five threads, I took a mean of the obſervations made with the circle, and another of thoſe made with the tranſit inſtrument. The difference between thoſe with the former inſtrument and thoſe with the latter, was never greater than 0·2 in time, except on the 19th of January, when I found $1''$ in time more by the circle. In regard to the declinations, I have found no corrections to be made. Should any aſtronomer, for his greater ſatisfaction, be deſirous to ſee the original obſervations, I ſhall be happy to communicate them to him. They will be publiſhed in the ſixth book of the *Specola Aſtronomica*, together with all my other obſervations made ſince 1794.

XIII. *Reflections on the new Primary Planet ſuppoſed to exiſt between Mars and Jupiter, and now in all Probability diſcovered. By Baron* Von Zach, *Lieutenant-Colonel in the Service of the Duke of Saxe-Gotha, and Director of the Ducal Obſervatory at Seeberg* *.

THE exiſtence of a primary planet, which, on account of the faintneſs of its light and ſmall ſize has hitherto remained undiſcovered, was, as far as I know, firſt conjectured, or at

* From *Monatlich Correſpondenz zur Beförderung der Erd-und Himmelſkunde*, June 1801.

leaſt

leaſt publicly mentioned, about forty years ago by the immortal Lambert. In his Coſmological Letters on the Univerſe, which appeared at Augſburg in 1761 *, the following remarkable paſſage occurs towards the end of the firſt letter: "And who knows whether ſome planets may not be loſt, from the wide ſpace between Mars and Jupiter?" This idea was no doubt ſuggeſted to Lambert by comparing the different diſtances of the planets; and he muſt ſoon have found that the diſtance between Mars and Jupiter is too great in proportion to the reſt. To fill up this vacancy he placed in it a new primary planet; and as it had not been obſerved during the courſe of two centuries, that is, ſince the invention of the teleſcope, he ſuppoſes it to have been torn from the powerful influence of the ſun by a deſtructive comet, round which it revolves in infinite ſpace as a ſatellite; and concludes with this obſervation: "Is the caſe the ſame among the celeſtial bodies as on the earth, that the ſtronger exterminate the weaker; and, are Jupiter and Saturn deſtined merely to prey upon others?"

What might tend more and more to confirm aſtronomers in the idea of the exiſtence of ſuch a planet, was a certain relation which they had obſerved in regard to the diſtances of the ſix primary planets from the ſun, and which was confirmed, in an unexpected manner, by a ſeventh primary planet beyond the orbit of Saturn diſcovered by Dr. Herſchel in 1781. This remarkable relation was firſt adopted by profeſſor Bode in the ſecond edition of his Introduction to the Knowledge of the Starry Heavens, publiſhed in 1772, from profeſſor Tilius's tranſlation of Bonnet's *Contemplation de la Nature*, Amſt. 1764. To repreſent this relation by ſmall numbers, which can be eaſily compared if the diſtance of Saturn from the ſun be divided into 100 equal parts, the diſtances of the planets from the ſun in ſuch parts will be as follows:

1. Mercury - 4 ſuch parts from the ☉
2. Venus - $4 + 3 = 7$
3. Earth - - $4 + 2.3 = 10$
4. Mars - - $4 + 2.2.3 = 16$
5. Hera or Juno $4 + 2.2.2.3 = 28$
6. Jupiter - $4 + 2.2.2.2.3 = 52$
7. Saturn - - $4 + 2.2.2.2.2.3 = 100$
8. Uranus - $4 + 2.2.2.2.2.2.3 = 196$, &c. &c.

or more generally the n^{th} planet, reckoning from the ſun, is

* A French tranſlation of theſe letters, by Darquier of Toulouſe, was printed at Amſterdam in 1801 by Hulſt van Keulen, with notes reſpecting the lateſt diſcoveries by J. M. C. Utenhove, of Utrecht. This edition was undertaken in conſequence of my recommendation.

diſtant

distant from that body $4 + \left(2^{n-2} \cdot 3\right)$: or, if we express, as professor Wurm has done*, the mean distance of the first planet by a, the difference between the distance of the first and second by b, and the mean distance of the earth from the sun by 1, the mean distance of the n^{th} planet from the sun will be $= a + \left(2^{n-2} \cdot b\right)$.

This law is founded on no known theory, at least it has never yet been demonstrated *mathematically*, and is merely deduced *empirically* from *analogical conclusions*. In no science have the powers of the human mind, merely by mathematical reasoning and the acuteness of geometrical reflection, produced so many, so certain, and so pure truths as in astronomy. When we consider the immensity and exalted nature of the objects on which this science is employed, and the insignificance of man and his terrestrial habitation, together with the endless variety and concatenation of the celestial phænomena, which all take place in consequence of one general very simple law of nature, that of gravity, diffused throughout the whole creation; and when we reflect what abstruse mathematical formulæ and methods must be invented to subject these so variously combined phænomena to calculation, and to obtain a uniform and continual correspondence of these calculations with what actually takes place in the heavens, it must be acknowledged, that no science does more honour to the human genius; that in no science have more discoveries been made *à priori*; and that no science is founded on more incontrovertible proofs than the exalted science of astronomy†.

The mathematical astronomers, for there are some who are not so, do not readily adopt any thing that cannot be mathematically proved. However great, therefore, might be the probability that the above-mentioned relation, in regard to the distances of the planets, taking it at least as an approximation, existed in nature, there were astronomers who doubted the consequences of this undemonstrated law, and the existence of an invisible planet between Mars and Jupiter.

* Berlin Astron. Jahre Buche, 1790, p. 168.

† La Place, that eminent analyst, in the preface to his excellent work, speaking of the progress of the modern chemistry, says: Et ce ne sera qu'au moment où les loix des affinités chimiques seront suffisamment observées, pour y appliquer l'analyse, que cette science (la chimie) aura le degré de perfection auquel l'astronomie s'est elevée par la decouverte de la gravitation universelle. *Théorie du Mouvement et de la Figure elliptique des Planètes*, 1784.

It is worthy of remark, that the aſtronomers of no nation but Germany adopted this conjecture in their aſtronomical books, or wrote any thing on the ſubject. How is this to be accounted for? Does the ſpirit of an eminent German, the ſpirit of a Kepler, ſtill hover over Germany? I do not here mean to aſſert that the Germans believed unconditionally in the exiſtence of this planet, or acknowledged it as proved. Profeſſor Bode has mentioned it in all his valuable works on aſtronomy, and in all the editions of them publiſhed ſince 1772; but he always ſpeaks of its exiſtence as founded on conjecture or analogy, and not as a demonſtrated truth.

About ſixteen years ago I employed myſelf in analogical calculations of the elements of the orbit of this latent planet, as may be ſeen by the letters I wrote to profeſſor Bode from Dreſden, in September 1785, which were printed in the Berlin Aſtronomical Almanac for 1789. But I ſpoke of theſe reſearches as mere *dreams*, and called my calculations *chimerical*. I even ridiculed them, and compared them to the reſearches of the adepts who endeavour to find the art of gold-making. In the year 1798, when, in conſequence of Lalande's viſit to Gotha, I had the pleaſure of ſeeing my highly eſteemed friend Bode, this ſubject formed part of our converſation. That worthy veteran Lalande had no great faith in the exiſtence of this planet; he has not ſaid a word of it in any of the three editions of his Aſtronomy which have been publiſhed: and I uſed to call thoſe who entertained a ſtrong belief of its exiſtence, *aſtronomical adepts*.

In the year 1787 profeſſor Wurm was engaged with ſimilar thoughts on the poſſibility of other planets and comets exiſting in our ſyſtem. Theſe ideas he communicated to the public in the Berlin Aſtronomical Almanac for 1790, and in that of the following year, where he extended them to the ſyſtem of the ſatellites. But he obſerved at the ſame time, that he was far from wiſhing to make any one adopt his aſtronomical *viſions*, as he expreſsly terms them.

Aſtronomers had the greater reaſon for being on their guard againſt analogical concluſions, as the remarkable inſtance of our great maſter may ſerve to ſhow that too much confidence muſt not be placed in a lively imagination. Poets may be allowed ſome latitude in this reſpect; but we muſt refuſe the ſame indulgence to a certain claſs of naturaliſts and philoſophers, who think they can catch nature in the ſlippery paths of myſticiſm; who ſubſtitute an unintelligible jargon for a language generally underſtood; and who attempt to explain a difficulty by obſcurity.

Kepler, who often indulged in ſuch aſtronomical dreams,

and gave full ſcope to his ardent imagination, thought he had made a great diſcovery by ſuppoſing that the five regular bodies would fit exactly into the vacuity between the ſix planets known at that time; and their diſtances indeed, according to the lateſt obſervations, correſpond very well with this rule. But unfortunately, as has been remarked by profeſſor Wurm, Euclid and Nature left no regular body for Uranus; and I may add, that none remains for Hera: ſo that Kepler's ingenious idea falls at once to the ground.

Should the queſtion propoſed on the diſcovery of Uranus be here aſked—Why was not this planet diſcovered long ago? we might give the ſame anſwer to it as given by Lichtenberg *, who conſidered it of the ſame kind as that of Lelio's ſervant in Leſſing's Treaſure, who wiſhed to know why his father had returned on a certain day, and not a year ſooner or later; which he thought would have been much leſs incomprehenſible.

The moſt natural way is, as profeſſor Bode has done in his Illuſtration of Aſtronomy, to conjecture that this planet, being ſmaller than Mars, and at a conſiderable diſtance beyond it, reflects too little light from its ſurface; and hence it has eſcaped the keeneſt-ſighted obſervers. But who knows what the nature of its ſurface may be? We are acquainted with celeſtial bodies which exhibit different colours, ſhades of red and green; as for example Mars, and the double ſtar γ in Andromeda, the light of which increaſes and decreaſes; and others which even diſappear from our view.

Kant and Wunſch, in their Coſmological papers, aſſert therefore that this planet does not exiſt by itſelf, but that it is incorporated with Jupiter; which is therefore of greater ſize than it ought to be according to the ſuppoſed rule, and conſequently ſupplies the place of two planets. Kant aſcribes the ſmall ſize of Mars, and its want of ſatellites, to the ſame cauſe. But this hypotheſis was not neceſſary to explain the inviſibility of this planet, as it can be done in a much better manner, and more agreeable to the laws of nature. How long did Uranus remain concealed from our ſight! And yet it was not only in the heavens, but, as we now know, was ſeen and obſerved 20, 30, and 90 years before Herſchel's diſcovery of it, by a French, a German, and an Engliſh aſtronomer. How then could profeſſor Wunſch, in the ſecond edition of his Coſmological Converſations, publiſhed in 1791 †, conſequently ten years after the diſcovery of Uranus, aſk the following queſtion: "What kind of a body muſt that be which, though

* Götting. Taſchenbuch, 1783. † Vol. i. p. 599.

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ſo near, cannot be found by the beſt teleſcopes, though every ſmall ſpot in the heavens is every night examined with the greateſt diligence? But if this planet ſhould once be diſcovered, or if it be really diſcovered, it will be eaſily comprehended why, as a teleſcopic ſtar, it could have remained ſo long concealed among the innùmerable multitude with which the heavens are covered *. Profeſſor Wunſch thinks, that as the ſatellites of Saturn and Uranus, which ſhine only with a pale faint light, can be ſeen by means of good teleſcopes, this concealed planet might alſo have been ſeen. But profeſſor Wunſch does not reflect, that to ſearch for an exceedingly ſmall moveable luminous point, that changes its place in the immenſity of the heavens, is much more difficult than to ſearch for a ſatellite, which always remains in the neighbourhood of its primary planet, and muſt even be found in the field of the obſerver's teleſcope. As profeſſor Wunſch is a good mathematician, he may calculate the probability and poſſibility of the diſcovery of a ſatellite and of ſuch a planet. It is poſſible that this planet, as was the caſe with Uranus, may have been ſeen ſeveral times; but it is alſo poſſible that it is not always viſible. For, ſince it appears only as a teleſcopic ſtar during the time of its neareſt approach to the earth, at its greateſt diſtance from us it may eſcape the beſt inſtruments and diſappear entirely; which renders the finding of it ſtill more difficult and uncertain.

All theſe impediments might in part have been foreſeen; and it was only by an accident, or ſyſtematic regulation, that the diſcovery of this planet amongſt the immenſe number of teleſcopic ſtars was poſſible. In the year 1787, when I undertook, at Gotha, a new reviſion of the ſtarry heavens, my view was to ſearch for this planet, to which I was particularly encouraged by the auguſt founder of the obſervatory of Gotha. I therefore confined myſelf merely to the zodiacal ſtars, and made a catalogue containing the right aſcenſion of theſe ſtars, under a conviction that it was poſſible by theſe means alone to fall upon this concealed planet.

In the autumn of laſt year, when I had the pleaſure of undertaking a ſhort aſtronomical tour to Celle, Bremen, and Lilienthal, and of ſpending a few agreeable weeks in the company of ſome of the ableſt German aſtronomers, theſe

* On the diſcovery of Uranus Lalande made the following reflection: Le nombre des étoiles de ſeptieme grandeur eſt ſi prodigieux qu'on auroit regardé comme impoſſible et inutile de les obſerver toutes, et à pluſieurs repriſes; cela eût eté cependant néceſſaire pour ſavoir s'il n'y en avoit pas quelqu'un qui eût un mouvement. *Ephémer. des Mouv. celeſt.* vol. viii. p. 88.—Lalande here alludes to ſtars of the 7th magnitude, but Hera appears as a ſtar of the 8th or 9th.

eminent men were of opinion that it was not poſſible for one or two aſtronomers to examine all the teleſcopic ſtars of the whole zodiac in ſearch of this concealed planet. Six aſtronomers then aſſembled at Lilienthal reſolved therefore, on the 21ſt of September 1800, to eſtabliſh a ſociety of twenty-four practical aſtronomers diſperſed throughout different parts of Europe, for the expreſs purpoſe of ſearching out this planet ſuppoſed to exiſt between Mars and Jupiter. They elected Mr. Schröter as their preſident; and I had the honour of being choſen their perpetual ſecretary. The plan of the ſociety was to divide the whole zodiac among the twenty-four members, each of whom was to have aſſigned to him, by lot, a zone of 15° of longitude, and of from 7 to 8° of north and ſouth latitude. Each member was to conſtruct a new celeſtial chart of his department, comprehending the ſmalleſt ſtars, and, by repeated examination of the heavens, to aſcertain the true ſtate of his diſtrict, and whether it contained that wandering body. By this inſtitution we entertained hopes that this ſtar, which had ſo long eluded our reſearches, would be at length traced out. In the name of the ſociety I wrote to ſeveral of the moſt celebrated practical aſtronomers in Europe, inviting them to concur in this general object; and almoſt all of them agreed to the invitation with great readineſs *. Some of the members, who have already begun their labours with great diligence, have tranſmitted, for our inſpection, very intereſting information; and though our infant ſociety may be deprived of the honour of firſt diſcovering this planet, not only is the ſuppoſed diſcoverer of it among the number of its members, though the uncertain ſtate of conveyance by poſt or by ſea, in conſequence of the war, has hitherto prevented us from tranſmitting our invitation to Palermo, but the ſociety has already contributed a great deal, and will ſtill contribute more, towards correcting our catalogues of the ſtars: and as this is not the only object of the ſociety in the extenſive province of aſtronomy, its labours, by being continued, muſt be of conſiderable utility.

In the month of February, this year (1801), I received a letter from Lalande at Paris, in which he informed me, that Piazzi, aſtronomer at Palermo, had diſcovered, on the 1ſt of January, a ſmall comet in the ſhoulder of Taurus; it appear-

* The only aſtronomers who begged to be excuſed were profeſſor Sniadecki of Cracow, and profeſſor Wurm of Blaubeuern. The former, becauſe, the univerſity of Cracow being ſuppreſſed, he was conſequently obliged to leave the obſervatory: the latter, on account of the want of inſtruments. But this difficulty was removed by princely munificence. The duke of Gotha ſent profeſſor Wurm a 7 feet reflector by Herſchel, and the ducheſs an aſtronomical clock.

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ed like a ſtar of the 8th or 9th magnitude, without any nebula or tail: but as nothing elſe was ſaid of its poſition or courſe, it was not poſſible to find it; and therefore, in expectation of more correct information, I paid no further attention to it.

In April I received a letter from Bode of Berlin, dated the 14th of that month, in which he told me that he had received a letter from Piazzi, dated Palermo, January 24, ſtating, that on the 1ſt of January he had diſcovered a ſmall comet in 51° 47′ right aſcenſion, and 16° 8′ north declination. On the 11th of January, from being retrograde, it had become direct; on the 23d, its right aſcenſion was 51° 46′, and its northern declination 17° 8′. He hoped he ſhould be able to obſerve it during the whole of February; it was very ſmall, like a ſtar of the 8th magnitude, and without any nebulous light. Profeſſor Bode added: "On reading Piazzi's letter, I was much ſtruck with the appearance and motion of this ſuppoſed comet; and I immediately wrote to him to ſend me the ſequel of his obſervations. In the mean time I cannot help mentioning that I have found, by a well-known and eaſy calculation, that the two obſervations of the 1ſt and 23d of January, and the ſtationary ſtate of the ſtar on the 11th of the ſame month, agree excellently with the ſuppoſition that it is not a comet, but perhaps the hitherto unknown planet between Mars and Jupiter, taking its diſtance at from 2·75 to 2·80. What do you ſay of it? It is much to be regretted that we have not a third obſervation. But, as the ſtationary ſtate agrees very well with the given places, it has become to me a matter of great importance. Send me a few words by the next poſt reſpecting your opinion. I may err, and wiſh for information; but the correſpondence is very remarkable. Do you know any more obſervations reſpecting this ſingular comet?"

On reading this letter, I immediately had recourſe to my old calculations of the years 1784 and 1785, and ſhowed to profeſſor Paſquich, who was preſent when I received the letter, that my elements of the orbit of this planet, calculated from analogy in the year 1785, and inſerted in the Berlin Aſtronomical Almanac for 1789, gave as its diſtance from the ſun 2·82, and as its period of revolution 4·74 years, or 4 years 9 months. Profeſſor Bode, from Piazzi's obſervations, had found the diſtance 2·75, and the revolution the ſame as I had deduced from analogy, viz. 4 years 9 months. I immediately ſent an anſwer to profeſſor Bode's letter, and informed him, that my two elements of the orbit of this ſo long concealed planet, calculated, proviſionally, ſixteen years before, amidſt my analogical dreams, and which I had depoſited in his hands in a ſealed

note in October 1785 *, when I had the pleaſure of forming a perſonal acquaintance with this worthy friend at Berlin, correſponded perfectly with his own, and conſequently with thoſe of Piazzi. I was therefore not only of opinion that the ſuppoſed comet might be the inviſible planet ſo long ſought for in vain, but found that Oriani of Milan, from whom I received a letter two days after, entertained the ſame opinion, that this ſtar was the planet ſuppoſed to exiſt between Mars and Jupiter. Piazzi himſelf, even in January, (Piazzi's letter to Oriani is dated on the ſame day as that to Bode, viz. the 24th,) informed Oriani that he conſidered this ſtar, which he firſt ſuppoſed to be a comet, as a real planet. The honour, therefore, not only of having firſt diſcovered this ſtar, but that alſo of having firſt aſcertained it to be a planet, cannot be refuſed to Piazzi; and one might almoſt ſuſpect (though we can hardly aſcribe ſuch a motive to him) that he wiſhed to retain to himſelf likewiſe the honour of having firſt calculated its orbit, as he communicated his obſervations in ſo ſparing and imperfect a manner.

Profeſſor Bode announced this diſcovery and conjecture to the Royal Academy of Sciences at Berlin, and cauſed it to be publiſhed in the Berlin Gazette of May 12, the Jena Literary Gazette of May 6, and in the Hamburgh paper of the 13th, from which it was copied into other journals.

Two days after I received profeſſor Bode's letter, and before I could return an anſwer I received one from Oriani, dated April 7, in which he ſays: "I have received a letter from Piazzi at Palermo, which contains information highly worthy of your attention and of all aſtronomers. He writes me, that on the 1ſt of January 1801 he had obſerved a ſtar, of the 8th or 9th magnitude, in the ſhoulder of Taurus. On the 2d of January he found this ſtar had advanced *about* 3′ 30″ further towards the north, and about 4 min. towards ♈ Aries. The two following days, the 3d and 4th, he found nearly the ſame motion. On the 5th, 6th, 7th, 8th, and 9th, the heavens were ſo obſcured that he could make no obſervation. On the 10th and 11th he again ſaw the ſtar; and afterwards on the 13th, 14th, 17th, 18th, 19th, 21ſt, 22d, and 23d of January. From the 10th to the 11th its motion, from being retrograde, had become direct. He adds, that on the firſt day of obſervation (Jan. 1ſt) its right aſcenſion was 51° 47′, and its declination 16° 8′ north. On the 23d of January he found R. A. 51° 46′, and N. D. 17° 8′. He writes alſo, that he firſt announced this ſtar as a comet; but, as he always

* Theſe elements had been communicated alſo to the duke of Gotha, to count Bruhl at London, and Köhler at Dreſden.

obſerved it to be without any nebulous ring, and to have a very ſlow motion, he had ſeveral times conjectured it might be a planet. This letter, written on the 24th of January, was unfortunately 71 days on the road; it was therefore difficult, from the only two poſitions given by Piazzi, to conjecture the place of this wandering ſtar after ſo long a period. I have, however, endeavoured to take advantage of the circumſtance, that on the 10th of January the ſtar from being retrograde had become direct, and, on the ſuppoſition of a circular orbit, have found that its diſtance from the ſun muſt be three ſemi-diameters of the earth's orbit; ſo that this ſtar may be a new planet, the orbit of which will fall between thoſe of Mars and Jupiter. There is reaſon to think that the orbit of this planet, like thoſe of all the reſt, will have a perceptible eccentricity*, and conſequently the hypotheſis of a circular orbit, which I ſuppoſed, muſt be improper for repreſenting its motion and geocentric place after ſo long a period: we muſt therefore wait for the remainder of Piazzi's obſervations, which he no doubt has continued. The heavens with us, ſince the receipt of Piazzi's letter, have been always overcaſt: you, perhaps, have a ſky more favourable for aſtronomical obſervations than ours†. In this perſuaſion, I ſend you my elements of the orbit, calculated, as you will ſee, from theſe imperfect obſervations, by which you will be able to calculate the planet's place nearly. Heliocent. long. of the ſtar at noon, Dec. 31, 1800, $2^s\ 6°\ 54'$; heliocent. motion in long. in 100 days $18°\ 19'$. Longitude of the aſcending node, $3^s\ 8°\ 32'$. Inclination of the orbit, $3°\ 50'$. But, as already ſaid, theſe reſults are ſubject to great doubts; for they are founded only on two very imperfect obſervations, and on the very inſufficient hypotheſis of a circular orbit. I, however, flatter myſelf that this letter will ſoon reach you; and, before the ſtar be loſt in the ſun's beams, you will perhaps be ſo fortunate with your ſuperior inſtruments as to find it, and to be able to give me more correct information reſpecting it."

Immediately after I received Oriani's letter, I calculated the place of the planet with its elements, and ſearched for it in the heavens on ſeveral ſerene evenings; but, unfortunately, the information had arrived too late. This ſmall ſtar had already advanced too near to the ſun, ſo as to be loſt in its

* According to my *conjectural calculations*, a very great one, 0·14; the greateſt next to that of Mercury. Time will ſhow whether I was right.

† At the bottom of the Thuringian foreſts, two hundred per cent. worſe than Milan! How badly my friend was informed reſpecting our April weather! It is a common proverb at Gotha, that during the brighteſt and fineſt weather one muſt not go abroad without a great coat or umbrella.

rays and the vapour of the horizon. I afterwards found that Oriani, in conſequence of his great haſte and anxiety to communicate to me the intelligence as ſoon as poſſible, muſt have committed an error in the calculation of theſe preliminary elements, particularly iu the ☊, and in the inclination of the orbit. But even had there been no error I ſhould not have found this ſmall wandering ſtar; for when I received the information the obſcurity was too great, and the ſtar too near the horizon. Profeſſor Bode alſo, as he informed me in a letter dated May 12, had ſearched for it ſeveral evenings, but in vain.

As no hope now remained of being able to obſerve this ſingular and remarkable body till its return from the ſun in Auguſt or September, I made a calculation of its orbit as well as poſſible from ſo imperfect and ſcanty obſervations, not with a view of finding it again in the courſe of two or three months (for before that time I hoped Piazzi, by continuing his obſervations, would give us better and more determinate data and concluſions), but merely that I might have by me ſome conjectures to enable me with better grounds to believe in the actual exiſtence of a planet between Mars and Jupiter.

The obſervations given by Piazzi for calculating the orbit are partly imperfect and partly inſufficient. 1ſt, His two obſervations which are known, are only for minutes, and announced as *nearly*. 2d, For calculating the orbit of a planet or a comet, *three* obſervations at leaſt are neceſſary. 3d, The times of the obſervations are not aſſigned. In regard to the firſt, we may admit, that at leaſt the *neareſt* minutes of the obſervation are given right. In regard to the ſecond difficulty, Piazzi perhaps, as already ſaid, may have purpoſely withheld the *third* obſervation, that he might calculate firſt himſelf the orbit of this planet *; for he conſidered it as ſuch before the 24th of January. But if this were the caſe, he betrayed, in ſome meaſure, his having made a third obſervation; as he mentions that the planet from the 10th to the 11th of January was ſtationary. Oriani and Bode took advantage of this circumſtance, and I employed it myſelf to calculate an approximate orbit of this planet from theſe ſcanty obſervations. The third difficulty was obviated by the following conjectures:

Piazzi, as is well known, is employed in the conſtruction

* The celebrated French aſtronomer J. N. de l'Iſle went ſtill further. Having diſcovered, at the obſervatory of Paris, the long announced, long expected comet of 1759, he kept it a long time ſecret, obſerved it in private, and ordered his pupil Meſſier to ſay nothing of his diſcovery.

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of a large celeſtial chart *. He had alſo, in part, the good fortune which I hoped to enjoy while preparing my catalogue of the ſtars, and with the view of which the Lilienthal Aſtronomical Society was formed; and the planet would certainly not have eſcaped them, even if it had not been diſcovered by Piazzi †. Piazzi is furniſhed at his obſervatory with an excellent tranſit inſtrument, and a complete meridian circle by Ramſden, by means of which he no doubt fell upon this ſmall wandering ſtar. This ſtar muſt have culminated on the 1ſt of January, the day when diſcovered, about nine in the evening. At that time of the year and hour it is perfect night at Palermo; and therefore Piazzi could very well obſerve this ſmall ſtar, of the 8th or ninth magnitude, with a day telescope. It was, however, full moon; and as the moon had riſen three hours above the horizon of Palermo, the night muſt have been very bright. But the moon was four hours, or about 68° diſtant from the ſtar; and this circumſtance alone converts my conjecture into certainty, that Piazzi muſt have found this ſuppoſed comet not with a comet-ſearcher, but with his meridian inſtrument. The above-mentioned right aſcenſions of the wandering ſtar, converted into time, were conſequently the time of its culmination in ſidereal time: theſe I again converted into mean ſolar time, and thus brought out the true moments of the obſervations of the ſtar. By theſe, and the obliquity of the ecliptic 23° 28′ 10″, I have obtained the following data for calculating the orbit:

1801.	Mean Time at Palermo.	Geocent. Long. of the Plan.	Geocent. Lat. South.	Place of ☉ + 20″	Log. Dist. ☉ ⊕
Jan. 1ſt	8h 43′ 15″	1s 23° 29′ 40″	2° 37′ 5″	9s 11° 1′ 40″	9·9926158
23d	7 16 41	1 23 43 40	1 38 50	10 3 22 28	9·9932351

As the ſtar on the 10th of January became ſtationary, I found its elongation for that time 7s 26° 41′ 41″, and, by means of Keil's propoſition, that the tangent of the elongation is equal to the ſemidiameter of the orbit divided by the ſquare root of that ſemidiameter, + 1. I found this ſemidiameter = 3·071, and, by Kepler's principle, the revolution

* Laſt year he publiſhed his large catalogue, comprehending 5500 ſtars.

† It was really fortunate that Piazzi diſcovered his ſtar on this day; for, had he fallen upon it eight days later, it is poſſible it might have eſcaped him; for, as this wandering ſtar about that time became ſtationary, he would not, from the obſervations of next day, have remarked its motion. It is very ſingular that this new planet, if its being ſuch be really confirmed, was diſcovered and ſeen in the ſhoulder of Taurus, as Uranus was 20 and 110 years ago.

$(3{\cdot}071)^{\frac{3}{2}}$

$(3{\cdot}071)^{\frac{3}{2}} = 5{\cdot}3817$ years. By the usual methods I obtained the following approximate elements of the orbit:

Epoch of the mean heliocentric longitude for the commencement of 1801 -	2^s	6°	55′	40″
Long. of the ascending node - -	2	22	0	50
Mean annual motion - - -	2	6	54	25
Inclination of the orbit - - -		7	47	40
Semidiameter of the orbit, 3·071				
Synodical revolution - 5·3806				

Those who attentively consider and compare these elements will no doubt remark, that the distance and periodical time of Piazzi's star are exactly the same as those of the celebrated comet of 1770, the calculation of which gave so much employment to astronomers, and the orbit of which Lexel could no otherwise represent than in an ellipse of $5\frac{1}{2}$ years. Pingré also found its distance = 3·09, and the period of revolution 5·4 years *. Burckhardt, who lately obtained the second prize proposed on this subject by the National Institute, could not reduce this singular body from new observations into any other than an elliptical orbit of $5\frac{1}{2}$ years. Was the comet of 1770 the so long concealed planet? or was Piazzi's star the comet of 1770? In either case, whether it was a comet or planet, Why was it not oftener seen, or before discovered? The causes, some of which we have already mentioned, may have been various. But to confine myself to a probable way of explaining this point, let the reader only look into Schröter's Observations on the Comet of 1799, published in the third volume of his Collections in regard to the latest Astronomical Discoveries †. The most singular phænomena of the accidental variable modifications of the nebulous light surrounding the comet, and also the well-known atmospheres of the planets, and even the sun, will be found there explained. In the nebulous light which surrounded the comet of 1799, Mr. Schröter observed not only occasional, but sudden and disproportionably great changes both in the extent and strength of the photosphere. Dr. Herschel not long ago presented to the Royal Society a paper on the structure of the sun, in which this eminent astronomer explains the solar spots by supposing an elastic and not luminous gas disengaged from the opake surface of the sun's body, which divides the surrounding luminous fluid or photosphere, by which means we then see dark parts of the body, which we consider as spots.

Is it not therefore possible that we see stars at one time

* Cometographie, part ii. p. 89.

† Allgem. Geograph. Ephemerid. vol. iii. p. 432.

which

which disappear at another? We are acquainted with many which have periodical changes of light of greater or longer duration; as, for example, those numerous changeable stars called *Stellæ mirabiles*. What has become of the celebrated Tycho Brahe's star in Cassiopeia of the year 1572, which at one time shone as bright as Sirius, and even exceeded Venus and Jupiter when in their perigeum; so that it could be seen in the day with the naked eye; since which time not the smallest traces of it have been found? We learn by the old chronicles, that in the time of the emperor Otho I. about the year 945, and also in 1264 *, a new and motionless star appeared between Cassiopeia and Cepheus. Some astronomers, therefore, conjectured that this might be the star of 1572, which appeared periodically in the course of about 300 years †. Had it any affinity with the celebrated star discovered by Kepler in 1604 in the foot of Serpentarius, which seemed at one time so bright that it exceeded in splendour stars of the first magnitude, became invisible next year, and has never since appeared? What are Herschel's planetary nebulæ? Did not Schröter ‡ find that whole tracts of luminous nebulæ disappeared from the heavens? What are our large fire-balls, which move with a velocity a hundred times greater perhaps than that of a cannon bullet, emit a bright light, burst, and then disappear? It is proved that they are bodies belonging to the universe, and not to our atmosphere. This was the opinion of Montanari § in 1676, who, in his *Fiamma volante* ||, calculates that one of these fire-balls, seen throughout all Italy in the above year, was at the height of forty Italian miles. All our readers must remember the large fire-ball seen in the year 1783 in every part of Europe, and pretty well observed. It was calculated by some of the English astronomers that the height of this immense body of fire was 60 English miles, and that its greatest diameter was $1\frac{1}{4}$th mile. In seven minutes it would have passed over a space equal to the semidiameter of the earth.

Why may not comets appear sometimes luminous and at

* Leovit. apud Tychon. lib. i. p. 441. Licet. lib. v. cap. 12.

† See Laplace's hypothesis on this subject in his *Exposit. du Système du Monde*, liv. v. p. 347. an. 7. the only real edition in quarto acknowledged by the author.

‡ Beytrage, p. 232.

§ Halley and Hevelius considered them also as cosmical bodies.

|| La Fiamma volante, gran Meteora veduta sopra l'Italia la sera de 31 Marzo 1676, Speculazioni fisiche et astronomiche espresse dal Dott. Geminiano Montanari ..... in una Lettera all, Ill. Ecc. Sgr. Marchese Federico Gonzaga, Bologna 1676. 95 pages quarto: a very scarce work, for which I am indebted to the kindness of professor Blumenbach, and which Dr. Chladni, who has collected on this subject with so much care, was never able to obtain.

others

others dark? The comet of 1770, therefore, might exist sometimes in an opake, and sometimes in a phosphorescent state; and hence perhaps, and from the perturbative power of the larger and more dense bodies, the unfrequency of their return may be explained. They come back, and we do not see them; they are present, and we do not observe them. To draw conclusions, from the identity of the elements of the orbit of a body, respecting the identity of the body itself, gives, as all astronomers know, but a high degree of probability*, among the ninety-one comets, confirmed by only one hypothesis †, which certainly may be true taken *directly* but not *inversely*. Two comets, however, whose orbits have not the identical same elements, may be one and the same body. This, as far as I know, has never been explicitly asserted by any one: but Lexel mentioned it implicitly, when the following objection was made to him on account of the five years elliptical orbit of the comet of 1770: Since the period of the revolution of this comet is so short, why was it not seen oftener, and long ago? Lexel was of opinion, and perhaps so was the great Euler his master, under whose inspection he laboured, that the influence and perturbation of the large body of Jupiter, to which this comet approached very near on the 27th of May 1767, and the 23d of August 1779, might have entirely changed its orbit ‡. Burckhardt was of the same opinion in his memoir written on the subject of the prize already mentioned.

But how frightful is the mere idea of calculating the perturbation of such a body! Would not such a calculation exceed the powers of our analysis? The difficult theory of the moon would be mere elementary calculation compared with the variable orbit of such a body. It were to be wished that so striking a phænomenon would produce the necessity of giving to our calculation, in regard to perturbation, a new direction, that the theory of so complex an approximation might be more improved, and that the influence of the successive integrations on neglected quantities might be better and more accurately determined. But the geometrician, who can represent all the co-ordinates of the motion of each celestial body in speedily approximating series of sines and cosines of the angle depending on its real motion, does not, perhaps, exist on our earth.

* Monat. Correspond. vol. iii. p. 414

† The so called Halley's comet, the fifth and last observed return of which took place in 1759, and which will appear again in 1835.

‡ "M. Lexel pense, que son orbite peut avoir été *totalement* changée par l'action de Jupiter." Fingré Cometograph. part ii. p. 90. See also Mém. de Paris 1776. p. 648.

Many no doubt will conſider the tail which, according to Meſſier's expreſs obſervation *, attended the comet of 1770, as a proof that it could not be a planet. But, is it proved that planets cannot have a tail? We have planets with ſatellites, and others without theſe attendants; why therefore ſhould there not be planets with tails, to prove, in a viſible manner, that they are nothing elſe than planets? This difference in the appellation originated in the periods of ignorance, and muſt now be admitted into our language to diſtinguiſh thoſe bodies, the appearance of which is of ſhorter duration, and which do not remain viſible throughout their whole orbit †, from thoſe which are always viſible, except when they approach too near to the ſun. The circumſtance which ſeems to be peculiar only to comets, that ſome of them move retrograde, while all the planets move direct, is only apparent. The reader is referred to the explanation which Laplace and Lalande have given of this point in regard to the retrograde ſatellites of Uranus ‡. Lalande ſays the word *retrograde* impoſes by its expreſſion, but in reality is nothing. Kant, in his *Allg. Nat. und Theorie des Himmels*, original edition, publiſhed in 1755, conjectured that the retrograde motion of ſome comets might be only an optical illuſion, like that of the geocentric motion of the planets.

Every newly diſcovered object muſt have a new appellation. Though a name in itſelf is of no great importance, we have ſeen in regard to Uranus how difficult it is for all mankind, from the Thames to the Neva, to be unanimous in this reſpect. If the ſtar lately diſcovered by Piazzi be really the ſuppoſed planet between Mars and Jupiter, a great and auguſt patron of aſtronomy, the founder of the obſervatory of Seeberg, gave it, in my opinion, a very appropriate name fifteen years ago. Uranus has afforded us a ſtrong right, on the ſcore of uniformity, to aſſign to this new planet, as has been the caſe with the old ones, a name borrowed from the heathen mythology. The duke of Gotha propoſed, therefore, that of *Hera*, Ἥρα, or of the deity ſtyled by the Romans Juno, the daughter of Saturn, and the ſiſter and wife of Jupiter §: Jupiter therefore would have his father and grandfather above him, and his wife and children below him.

* Mém. de Paris 1776. p. 597.

† The new planet, perhaps, is not viſible in apogeum, as already obſerved.

‡ Allgem. Geograph. Ephemerid. vol. ii. p. 170 and 259. See alſo Laplace's *Expoſit. du Syſtème du Monde*, p. 342.

§ Called alſo *Saturnia*. The duke at firſt propoſed *Rhea*, the wife of Saturn; but this name has already been applied to the Earth.

The

The Greek name *Hera* is to be preferred to the Latin name Juno, 1ſt, Becauſe the latter has been already applied to the planet Venus. Pliny ſays *, "Infra ſolem ambit ingens ſidus, appellatum Veneris . . . . . Alii enim *Junonis*, alii *Iſidis*, alii matris Deûm appellavere." St. Auguſtine † calls Venus, Stellam *Junonis*; and Apuleius ‡ ſays, "*Junonia*, immo Veneris ſtella cenſetur." *Hera* is always involved in clouds, and our planet kept itſelf a long time concealed. This name, therefore, will ſtill be appropriate even if the new ſtar ſhould not be the ſuppoſed planet: in that caſe, inſtead of the deity we have embraced a cloud §. 2d, *Hera* is at the ſame time the name of a city in Sicily, by which means the diſcovery made on that iſland, and the celebrated name of the diſcoverer of this eighth primary planet, will be eternized and preſerved as long as tradition and hiſtory exiſt on the earth. The city of *Hera*, ſituated, as well as Palermo, on the coaſt of Sicily, was called alſo *Hybla Minor*, and is the ſame mentioned by Pauſanias, Cicero in his Letters to Atticus, and Antoninus in his Itinerary. In the laſt place, *Hera* is the mother of Vulcan, who has his workſhop in the burning mountain of Ætna in that iſland.

The objection already made in regard to Uranus, that all the planets have Roman and not Greek names, muſt be of leſs importance, as the Greek name Οὐρανὸς has been retained to that diſcovered by Herſchel, though it would have been more agreeable to analogy to have called it *Cœlus*. All the old planets, the diſcovery of which is loſt in the obſcurity of time, may retain their Latin names; but the new planets, the hiſtory of the diſcovery of which will be handed down to the lateſt poſterity, with the names of the diſcoverers, ought, by way of diſtinction, to have Greek names. What ſeems here an interruption of analogy will be only harmony. Since the creation, like the Creator, has no bounds, ſhould another planet be diſcovered beyond Uranus, its hieroglyphic appellation ought to be Greek.

It will be neceſſary alſo to invent an appropriate character for this planet. To a new planet we may aſſign the character of a new metal. This idea has been followed in regard to Uranus; but we have thereby committed an error, or rather eſtabliſhed a monument of our ignorance reſpecting the component parts of platina. It was a much better propoſal to diſtinguiſh this planet by ♅, the under part of which repre-

* Hiſt. Nat. lib. ii. cap. 6.
† De Civitat. Dei, lib. vii. cap. 15.
‡ De Mundo, p. 252. edit. Bipont.
§ Nubes et inania captare.

ſents

fents a planet, and the upper a fixed ſtar: it ſhould, indeed, denote a fixed ſtar become a planet. But as this character has been already introduced in the *Vienna Ephemerides*, in order to prevent miſconception it ought to be inverted. The characters of the Earth ♁, and Venus ♀, have the ſame affinity, as well as thoſe of Mars ♂ and Uranus ♅.

POSTSCRIPT.

While the laſt ſheet of this paper was printing we received from our worthy friend Dr. Olbers, of Bremen, the following elements of the orbit of Hera, which he calculated in a circle from the obſervations communicated to him by Piazzi:

Semidiameter of the orbit - -	2·951
Longitude of the aſcending ☊ -	2ˢ 21° 55′
Inclination of the orbit - -	7 56
Heliocent. long. Jan. 1ſt, 1801 -	2 7 45
Sidereal revolution -	1851·6 days = 5·0694 years.
Daily heliocent. motion - -	11′ 39·95″
Yearly motion - - -	71° 1′

This orbit is very inſufficient, as it is calculated on the hypotheſis of a circle: the very imperfect obſervations were only 22 days diſtant from each other; and, as Dr. Olbers very well obſerves, the lines of ſight do not lie advantageouſly. Dr. Olbers is alſo of opinion that theſe elements are not ſufficient to calculate a planet ſo long before, in order to be able to find it again on its reappearance in the morning in Auguſt. In the month of Auguſt it will paſs through the whole ſign of Cancer, and muſt be ſought for in the equinoctial point. From the 1ſt to the 31ſt of Auguſt, a zone of the heavens from 115° to 130° of right aſcenſion, and from 22° to 24° north declination, muſt be carefully ſearched; for this will be the diſtrict of the heavens where the planet will, in all probability, be found in that month.

In the oldeſt periods Latin verſes have been invented to denote the order of the planets in regard to their diſtance from the ſun. Thus, for example, we have the old well-known verſes:

Saturni atque Jovis Sidus, Mars, Sol, Venus alma,
Mercurius, claudit ultima Luna chorum.

On the diſcovery of Herſchel's planet, Poinſinet de Sivry wiſhed to have it called *Cybele* or *Cybelle*, after the wife of Saturn, the neareſt planet below it; and expreſſed the order of the ſeven planets in the following three Latin verſes:

Ambit Solem Hermes, Venus hunc, mox Terra, Diana,
Mars ſequitur. Pergit rex Jupiter. Hunc Saturnus.
Omnes hos orbes amplectitur alma Cubelle.

One

One of my friends, possessed of a poetical vein, has expressed the order of the planets, now increased to eight, in the following four lines, and not unhappily:

Mercurius primus: Venus altera: Terra deinde,
Mars posthac: quintam sedem sibi vindicat Hera.
Jupiter hanc ultra est. Sequitur Saturnus: at illum
Uranus egreditur, non ausim dicere summus.

XIV. *Some Particulars respecting the new Planet* Ceres Ferdinandea.

IN the two preceding papers our readers are put in possession of every thing relative to the discovery of this planet. The following are some of the observed places of the Ceres Ferdinandea, upon its being rediscovered lately, and the times noted are mean time.

1801.

		M. T.	R. A.	Dec. N.
Dec.	7.	at $18^h\ 48'\ 10''$	$178°\ 33'\ 31''$	$11°\ 41'\ 30''$

1802.

Jan.	5.	By Dr. Olbers, at Bremen,		
		17 30	185 43	11 8 0
	11.	Again, by Zach,		
		at 17 3 17	186 45 50	11 10 0
	26.	By Mechain, at Paris,		
		at 16 10 48	188 24 49	12 0 43
Feb.	3.	By the astronomer royal, Greenwich,		
		at 16 11 15	188 43 0	12 39 0
	4.	at 17 25 46	188 43 0	12 44 0
	12.	at 15 4 12	188 30 0	13 33 0

On the 7th of February and subsequent days, it was observed by Dr. Herschel at Slough, and also by Alex. Aubert, esq. at his observatory at Highbury House.

The state of the weather at Slough did not admit of Doctor Herschel's seeing the new planet till Monday night the 8th current, he having previously received distinct notice as to its place from his friend Dr. Maskelyne, the astronomer royal, who for the first time observed it, like a star of the eighth magnitude, on the 4th current, in the morning. On the 9th and 10th of February at night, Dr. Herschel again traced the planet, and perceived its motion. It was not however till the morning of the succeeding day, that through a much clearer air, and at a more favourable altitude, with less obstruction

obſtruction from the moon-light, he obtained a diſtinct view of it for a ſhort time. Through his ten feet reflector, with a magnifying power of ſix hundred, and higher powers, he perceived its diſc, though very ſmall, yet diſtinctly round and well defined; but ſaw nothing further which denoted a ring or a coma, or a ſatellite. The favourable ſtate of the air appearing then very precarious, Dr. Herſchel did not attempt to aſcertain the apparent diameter of the planet by means of his lamp-micrometer, as ſome preparations and adjuſtments would have been neceſſary before he could have availed himſelf of that curious apparatus. But in order to form ſome eſtimate as to a point of ſo much importance, he adopted a happy expedient, and very ſuitable to the urgency of the moment. The Georgian planet being ſituated near at hand, in the ſame region of the heavens, he directed his teleſcope firſt to it, and then to the other, with his attention fixed upon making a comparative eſtimate of the apparent diameter of each diſc. In order to this, and to avoid as much as poſſible certain fallacies to which this method is more or leſs liable, he was careful to form eſtimates over and over again, according as his teleſcope was laſt turned from the greater diſc to the ſmaller; and *vice verſa*. From ſuch obſervations, frequently repeated, he concludes that the apparent diameter of the *Ceres Ferdinandea* is about a fourth part only of the apparent diameter of the Georgium Sidus. By applying therefore the proper calculation, Dr. Herſchel has inferred that the real diameter of this newly found primary planet, called *Ceres Ferdinandea*, is only a little more than half of the diameter of our moon and leſs than 5-8ths of it. The ſmallneſs of the planet, together with the great inclination of its orbit, are peculiarities which may probably lead to other diſcoveries in the ſolar ſyſtem.

The following are ſome places of the planet calculated forward in foreign journals, but corrected by the lateſt obſervations, ſhowing nearly where the planet may be expected to appear: computed for 12^h or 15^h Greenwich time.

1802.	R. A.	Dec. N.
Feb. 17.	188° 15′	14° 8′
23.	187 39	14 49
March 1.	186 50	15 31

The planet will be in oppoſition to the ſun about the 13th of March.

The following are the elements of the planet as calculated by M. Gauſs, of Brunſwick, for an elliptic orbit.

Epoch 1801, Jan. 0, or Jan. 1, which of the two is uncertain - - - 2^s 17° 36′ 34″

Aphelium - - -	10ˢ	26°	27′	38″
Aſcending node - - -	2	21	0	44
Inclination of orbit - -	0	10	36	57

Eccentricity 0,0825017 to its mean diſtance unity.
Mean diſtance from the ſun 2·7673.
Mean diurnal heliocentric and tropical motion, ſay, 12′ 50.914″.

Periodic time 1681 days, or 4 years 7 months.

Baron Von Zach, director of the obſervatory of Gotha, writes as follows to C. Mechain, adminiſtrator of the obſervatory of Paris:

"M. Schröter of Lilienthal has ſeen, with his large teleſcopes, the new planet Ceres, under a diſc of nearly 2″. He ſuſpects that it has two ſatellites. The planet is enveloped in a very thick atmoſphere, for it appears to be ſurrounded with much nebuloſity. I am very anxious to learn what Dr. Herſchel will tell us reſpecting it: in the mean time I thought it my duty to write you this in haſte."

Elliptic elements of the new planet, corrected by M. Gauſs from his laſt obſervations:
Diurnal tropical heliocentric motion 770·7376″.
Tropical revolution, $1681^{d}\ 12^{h}\ 9^{m}$.

It is a curious circumſtance that the diſcovery of this planet has been long expected, and even in ſome meaſure predicted. Profeſſor Bode, of Berlin, in his *Kurzer Entwurf der Aſtronomiſchen Wiſſenchaften*, Berlin 1794, § 387, has the following paſſage:

"Is it probable that Uranus, or the Georgian planet, is really ſituated at the utmoſt limit of our ſolar world? This appears to be very doubtful, conſidering the immenſe ſpace interpoſed between it and the neareſt fixed ſtars. Other planets perhaps may be ſtill more remotely ſituated, and may perform their revolutions unſeen by human eyes. We can ſcarcely ſuppoſe that any planet exiſts nearer to the ſun than Mercury; but conſidering the proportions of the diſtances of the planets from the ſun, we obſerve between Mars and Jupiter, a diſtance far greater than a compariſon of the other diſtances would lead us to expect, and this ſpace may perhaps be occupied by a planet yet unknown."

Similar ideas ſeem to have been entertained in this country, even long anterior to the conjectures of Lambert and the German aſtronomers, as appears by the following, which is given as a note in a work lately publiſhed on aſtronomy, by Mr. Olinthus Gregory, teacher of mathematics, Cambridge.

"Mr. Maclaurin and other philoſophers expected nearly one hundred years ago, that ſuch a diſcovery as this of

Piazzi would be made by ſome diligent aſtronomer; and the opinion has been lately revived by Mr. Capel Lofft, a gentleman well known for his attachment both to the ſciences and the muſes. In the New London Review for March 1800, this gentleman, in a critique on the Athenian Letters, ventured to offer ſome conjectures reſpecting an intermediate planet between Mars and Jupiter, the coincidence of which with the new diſcovery is very remarkable. He ſuppoſed that the diſtance of the intermediate planet from the Sun would be to that of Mars, either as 33 to 15, or as 20 to 15, the midway between which correſponds nearly with the fact. With reſpect to its diameter, he conceived it might be to that of Mars, as that of Mars to the diameter of the earth; and then being not much more than half the diameter of Mars, and at five times the perigean diſtance, it would be ſeen from the Earth under an angle of $2\frac{1}{2}''$ or $3''$, while Georgium Sidus would appear under an angle of $4''$. Theſe lucky conjectures were drawn from a certain kind of *Pythagorean harmony*, and are ingeniouſly defended in the Review juſt mentioned."

XV. *Deſcription of a new-invented Aſtronomical Inſtrument, for placing Globes in a proper Situation, by means of the Sun, without the Help of a Magnetic Compaſs, or other Inſtrument. By Mr.* B. M. Forster *.

IT conſiſts of a braſs circle, C,C, (Plate II. fig. 2 and 3.) made to fit the ſurface of a twelve-inch globe. Acroſs this circle is a braſs bar, B, B, arched on the under part, to fit the curve of the globe, and flat at the top. The ſides of this bar are graduated, anſwering to ten degrees of the ecliptic; and the under part is chamfered ſo, that the edge may be very narrow which lies over the ecliptic, when in uſe. On the flat top of the bar is a ſmall hole, with a metal pin S, ſerving as a *ſtyle* to caſt a ſhadow. The part which receives the ſhadow is painted a dead-white, to prevent the dazzling which the braſs alone would have occaſioned.

This little inſtrument, which may be called a ſolar ſtyle, was made under my direction by Mr. Blunt of Cornhill.

Method of uſing the Inſtrument.

Find the ſun's place in the ecliptic, by an almanac, (or on the wooden horizon of the globe, if marked there,) then place the inſtrument in ſuch a manner on the ſurface of the globe, that the pin or *ſtyle* be exactly over that place on the ecliptic. Having done this, turn the globe into ſuch a

* Communicated by the author.

ſituation that the *ſtyle* ſhall caſt no ſhadow: when this is the caſe the globe will be properly placed; or, as it is uſually called, rectified for the time of day.

This inſtrument may alſo be uſed for taking diſtances of places or ſtars, on different parts of the globe, which are not more than ten degrees aſunder.

XVI. *A few Particulars reſpecting Mr.* BOAZ's *Patent Telegraph*.*

THIS Telegraph exhibits the alphabetic or other characters, or ſignals, by means of lamps rendered viſible and inviſible by a mechaniſm ſimple in its conſtruction and immediate in its effects, as alſo by boards or other opaque bodies, which may, in like manner, be rendered viſible or inviſible at pleaſure, by alternately preſenting their fronts or their edges to the eye. Or by lamps and boards ſo combined together, that both or either may be uſed: but perhaps the modes eaſieſt in fitting up and application are the following, viz.;

Twenty-five lamps are placed in a frame at equal diſtances from each other, as repreſented by the white dots in fig. 1, No. 1, (plate III) each having a moveable cover, or blind, fitted to it, in ſuch a manner that, when they fall down, (which they do by their own weight) the lamps, or rather the flames of the lamps, are intercepted and rendered inviſible, ſo preſenting a mere blank to the eye placed in front of the frame, as in fig. 2, No. 1.

Another frame is provided having grooves cut in it: and for each letter of the alphabet or other ſign wiſhed to be exhibited, an oblong piece of wood is alſo provided, made ſo as to ſlide backward and forward in theſe grooves, having an alphabetical letter or other ſign, conſpicuouſly marked on the outer end of it. Any one of theſe pieces of wood, by being puſhed in or drawn out, pulls *all at once* from before the lamps, or lets down (by means of ropes or wires) as many lamp-covers or blinds as are neceſſary to exhibit that particular letter or ſign marked on that particular piece of wood, ſo puſhed in or drawn out. The letters of the alphabet ſo exhibited, are repreſented by figures 3 to 28, No. 1.

The ſame thing, however, may be accompliſhed by means of nine lamps only. Fig. 1, No. 2, repreſents the nine lamps, rendered viſible by having their covers or blinds removed as mentioned above. Fig. 2, No. 2, repreſents the whole covered. Figs. 3 to 28, No. 2, repreſent the whole

* Communicated by Mr. Boaz.

alphabet.

alphabet. The letters are perhaps leſs diſtinct than thoſe produced by means of twenty-five lamps; but this ſpecies of Telegraph comes cheaper, and is eaſier fitted up: and although the alphabetical letters repreſented by it appear at firſt ſight imperfect, yet, after having ſeen them twice or thrice, they will always after be eaſily recogniſed by the intelligent mariner or ſoldier, and may, even at firſt ſight, be read by one quite unacquainted with the invention. With twenty-five lamps, *capital* letters are exhibited: with nine lamps, *ſmall* letters muſt ſometimes be ſhown.

Forms of Signals ſuggeſted by the Patentee.

As only one letter appears viſible at a time, to divide theſe letters therefore into words, a ſign is requiſite; for which purpoſe fig. 29, No. 1 or 2, in the reſpective Telegraphs, is exhibited.

When a veſſel approaches another, and wiſhes to ſpeak the ſtranger, fig. 30, No. 1 or 2, is exhibited, being as much as to ſay "I wiſh to ſpeak you."

When the ſtrange veſſel ſees this, ſhe ſhows fig. 31, No. 1 or 2, as much as to ſay "I perceive you wiſh to ſpeak me, am ready to hear you;" that is, to mark down the letters you exhibit. When the veſſel has ſpoke, the ſtranger, in her turn, exhibits fig. 30, No. 1 or 2, and replies.

Fig. 32, No. 1 or 2, repreſents a ſign which the captain of the Eliza throws up, ſhould he wiſh to ſpeak the captain of the James, *without what he ſays being known to the crews of either veſſel.* For this purpoſe two circular flat rings of wood or metal may be put in a box, the one moveable on its centre, the other fixed. On one of theſe rings let the twenty-ſix letters of the alphabet be engraven, painted or written, at equal diſtances from each other, *progreſſively as they occur in the alphabet.* On the other ring let theſe characters be engraven, painted or written, *but irregularly.* Let each captain be poſſeſſed of a box as above, ſimilar in every reſpect.

When the captain of the Eliza throws up ſignal marked fig. 32, No. 1 or 2, and immediately thereafter the letter G, this ſhows he wiſhes to ſpeak privately to the captain of the James, on this letter (or key, as it may properly be called). On ſeeing this, the latter turns round the moveable ring in his private ſignal-box, until the two G's are over againſt or cloſe to each other, as in the figure annexed; (the white circle with black letters repreſenting the moveable ring;) it being underſtood that the captain of the Eliza has previouſly done the ſame thing in his private ſignal-box.

Every letter the Eliza's captain exhibits is taken down and deciphered or tranſpoſed by the private ſignal-box of the other. Thus, making a ſuppoſition that the arrangement of the alphabetical characters is as in the annexed repreſentation of the ſignal-box; if one exhibits the letters *drmw—ub—irpqr*, when tranſpoſed they expreſs the words *news of peace*; the letters ſo exhibited being divided into words, by fig. 29, No. 1 or 2, as above mentioned.

Fig. 33, No. 1 or 2, ſignifies a contraction of the conjunction *and*.

Fig. 34, No. 1 or 2, an apoſtrophe, neceſſary in writing French.

Fig. 35, No. 1 or 2, as much as to ſay, "I am in diſtreſs," or "I did not underſtand your laſt ſignal or ſentence, I pray you ſpeak again."

Fig. 36, No. 1 or 2, a period.

Fig. 37, No. 1 or 2, a final ſtop.

Or both 36 and 37, No. 1, may be given at once, ſignifying "I have finiſhed what I intended to ſay, you have now an opportunity to ſpeak without confuſing me."

Any two or more ſigns or characters can be exhibited at once, thus increaſing the number of appearances, which may be uſed as different ſignals.

Fig. 38. a ſalutation when the veſſels part, q. d. "farewell."

Figs. 39 to 48, Nos. 1 and 2, exhibit the numerical figures neceſſary to tell the latitudes, longitudes, &c. at ſea. With the nine-lamp Telegraph, however, the numerical figures are more liable to be miſtaken, and it might perhaps be as well rather to *ſpell the number* wiſhed to be expreſſed. This lies in the option of the ſpeaker.

Theſe are a few hints concerning this invention, which may be applied to a great variety of purpoſes. By uſing lenſes before, or reflectors behind each of the lamps, to increaſe the glare and render the light more vivid, they become viſible even during the brighteſt ſun-ſhine, at a very great diſtance.

At ſea the Telegraph hangs on pivots, to yield to the ſwing of the veſſel; ſo remains always perpendicular. For land ſervice this is unneceſſary, but in either caſe it may be turned round ſo as to face in any direction.

Thus a complete ſyſtem of communication may be eſtabliſhed,—ſimple, eaſy, and unſhackled;—not liable to be miſtaken, but legible by day or night to all;—capable of being the vehicle of ideas in any language, and ſo quick, that, if neceſſary, ſixty ſigns can be exhibited, and diſtinctly read at a great diſtance, in the ſpace of one minute.

This

This mode of communicating thoughts, information, and intelligence would conſequently be of much uſe on light-houſes, dangerous coaſts, harbours, fortifications,—at head-quarters, and out-poſts,—in ſhips of war, revenue cutters, merchant veſſels, &c. Correſpondence in this manner might alſo be carried on between gentlemen's country ſeats, if in ſight of each other, though far diſtant.

Orders for theſe Telegraphs, or offers from tradeſmen to make them, may be addreſſed per poſt "James Boaz, Glaſgow, North Britain;" or to Mr. Wyatt, No. 182, Fleet-ſtreet, London.

LONDON,
February 1802.

XVI. *Notices reſpecting New Publications.*

A Treatiſe on Aſtronomy, in which the Elements of the Science are deduced in a natural Order, from the Appearances of the Heavens to an Obſerver on the Earth, demonſtrated on Mathematical Principles, and explained by an Application to the various Phænomena. By OLINTHUS GREGORY, Teacher of Mathematics, Cambridge. London, Kearſley 1802. 8vo.

ASTRONOMY is a ſcience not only noble and exalted in itſelf, but attended with ſo many advantages that little needs be ſaid in its favour. Its utility in navigation, by which means that art has been brought to a ſtate of very great perfection, is alone ſufficient to ſhow its importance, and conſequently the benefits that muſt ariſe to ſociety by encouraging and promoting it. Moſt of the civilized countries in Europe, indeed, have lately paid conſiderable attention to this important object. And the great improvement made in the conſtruction of teleſcopes, and other aſtronomical inſtruments, has produced diſcoveries, and an accuracy in obſervations of the celeſtial bodies, which otherwiſe could not have been expected. By the munificence and liberal protection of our moſt gracious ſovereign, who has always ſhown himſelf a friend to ſcience, theſe improvements have been carried to a very great extent in this country: and it muſt fill the breaſt of every patriotic Briton with honeſt pride, to reflect that there is ſcarcely an obſervatory in Europe which is not furniſhed with teleſcopes by Herſchel or Dollond, and with quadrants, equatorial ſec-

tors, tranſit inſtruments, &c. by Ramſden and other eminent Engliſh artiſts. But though aſtronomy has been ſo much improved in this country, we were far behind ſome of our neighbours on the continent, in regard to good works on the ſubject. We do not here mean to throw out any reflections againſt thoſe authors who have written on aſtronomy, many of whom are juſtly entitled to praiſe. But we are of opinion that none of theſe works, the ſyſtem of profeſſor Vince excepted, is ſuited to the preſent improved ſtate of the ſcience; for works of aſtronomy, from the nature of the ſubject on which they treat, the neceſſity of frequently correcting the tables, and other circumſtances, muſt have a very limited period of utility. As profeſſor Vince's work is too bulky and expenſive for the great maſs of the public, it gives us pleaſure to find that a gentleman ſo well qualified for the taſk as Mr. Gregory ſeems to be, has turned his attention to this deficiency, and ſupplied the public with a comprehenſive, clear, and well arranged elementary treatiſe on this noble and uſeful ſcience. We have no heſitation in ſaying, that we conſider it as the beſt practical work on the ſubject publiſhed ſince the time of Leadbetter. The author's rules are ſimple and eaſy; and the whole rendered ſo familiar, by a variety of examples, that any perſon initiated in the principles of the mathematical ſciences muſt readily comprehend them. It will be of great utility to young perſons in particular, who are ſtudying aſtronomy; and thoſe who have made conſiderable proficiency will find it exceedingly convenient to refer to. The author has omitted none of the modern diſcoveries; and the tables he has given at the end are taken from the beſt ſources, and improved by the lateſt corrections.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

DR. GIBBES, in a paper on galvaniſm, read before the Society on the 21ſt of January, aſſumes an opinion contrary to that of Dr. Wollaſton, reſpecting the origin of electricity in chemical changes; and maintains, that the electrical changes are to be conſidered as preceding and favouring the chemical. He imagines that the ſimple contact of various ſubſtances produces changes of electrical equilibrium, and that the action of acids is effectual in promoting theſe changes by bringing their ſurfaces into contact. Dr. Gibbes obſerves

obſerves upon Dr. Wollaſton's experiment of immerſing zinc and ſilver in an acid ſolution, that if they are placed in two ſeparate portions of the fluid, and the parts not immerſed are brought into contact, there is no emiſſion of gas from the ſilver; but that it is copiouſly produced when the contact takes place in the ſame fluid. He proceeds to relate ſome experiments which ſeem to ſhow a difference between galvaniſm and electricity, particularly that galvaniſm does not appear to be attracted by metallic points. He alſo ſtates an experiment in which a piece of paper is placed on tinfoil, and rubbed with elaſtic gum; and although the tinfoil is not inſulated, ſparks are produced on raiſing the paper. Dr. Gibbes concludes with ſome arguments againſt the doctrine of the decompoſition of water; and advances as a probable opinion that oxygen and hydrogen gas are compoſed of water as a baſis united with two other elements, which combined form heat.

The meetings of the 28th January, the 4th and 11th February, were principally occupied by a paper on the hyperoxygenized muriatic acid, by Richard Chenevix, Eſq. F.R.S.

After a ſhort account of the experiments that had been made before him, and particularly of the ingenious conjecture of Mr. Berthollet *, Mr. Chenevix proceeds to ſtate the means by which he has aſcertained, that the acid contained in hyperoxygenized muriate of potaſh is muriatic acid in a particular ſtate of combination with oxygen, and the experiments by which he determined the proportion of theſe elements.

From the quantity of oxygen and of ſimple muriatic acid contained in the ſalt, he proves that hyperoxygenized muriatic acid is compoſed of

oxygen - -	65
muriatic acid - -	35
	100

From the proportions of the ſalt which is formed when a current of oxygenized muriatic acid is paſſed through a ſolution of potaſh, and which he found to be compoſed of the ſame elements, in the ſame proportion as oxygenized muriate of potaſh would be, if, at the very moment of its formation, it had not been reſolved into ſimple and hyperoxygenized muriate, he concludes that oxygenized muriatic acid is compoſed of

oxygen - -	16
muriatic acid - -	84

* *Journal de Phyſique*, 1788, page 217.

From

From a number of experiments which are ſtated at length, Mr. Chenevix imagines that the ſalts of the genus oxygenized muriate do virtually exiſt; but that, by ſuperior affinities, they are all reſolved into muriates and hyperoxygenized muriates at the very moment the acid comes in contact with the baſes.

He deſcribes and analyſes all the ſalts of the genus hyperoxygenized muriate; and mentions the hyperoxygenized muriate of ammonia as an extraordinary inſtance of diſperſing affinities. He obtained the alkaline ſalts pure by repeated cryſtallizations; and the earthy ſalts, by boiling them with phoſphate of ſilver.

He has examined likewiſe ſeveral of the metallic ſalts of this genus, and mentions hyperoxygenized muriate of ſilver as ſtrongly marking the difference between muriatic and hyperoxygenized muriatic acids. He aſcribes to this ſalt, when mixed with any combuſtible body, an expanſive force, which he thinks he will be within bounds if he ſtates to exceed five times that of any known detonating ſalt.

He concludes with an appeal to the chemical world, whether, in the preſent ſtate of the ſcience, it would not be more philoſophical to ſay,

Muriatic radical, or ſome one word of the ſame import,	inſtead of	Muriatic acid;
Muriatous acid,		Oxygenized muriatic acid;
Muriatic acid,		Hyperoxygenized muriatic acid;

and ſtates the arguments in favour of each appellation.

As a warning to thoſe who would repeat his experiments, he relates, in the courſe of his paper, an accident that happened in his laboratory, to himſelf, and to Mr. Vandeer, by which the latter gentleman had almoſt loſt his ſight, and was wounded in the moſt dreadful manner.

On the 4th and 11th of February, letters from Dr. Maſkelyne, Mr. Von Zach, Alex. Aubert, Eſq. and Mr. Gilpin, clerk to the Royal Society, were read, ſtating that they had obſerved the new planet. As we have already given its obſerved places, (ſee p. 54, &c.) we need not here repeat them.

On the 18th of February, a paper by Dr. Herſchel on the ſame ſubject was read. His reſults are given in the preceding pages.

ROYAL ACADEMY OF SCIENCES, BERLIN.

The laſt ſitting of the Royal Academy of Sciences was occupied

cupied a good deal with galvanic experiments. Mr. Herhard has found that nickel in contact with zinc produces the ſame effect as ſilver and copper. Klaproth read ſome account of the galvanic experiments, made on a large ſcale, by Van Marum, of Haarlem, and of his counter-trials with Tyler's large machine. Theſe trials confirm Volta's theory reſpecting the identity of galvaniſm and the electric fluid.

FRENCH NATIONAL INSTITUTE.

[Continued from p. 381, vol. xi.]

C. Meſſier having in vain ſpent the night of the 3d of October in ſearching for the new ſtar; to conſole himſelf for his want of ſucceſs, he began to conſider the very uncommon ſpectacle exhibited by the conſtellation of the Lion, in which Saturn, Jupiter, Venus, and the Moon were collected around the beautiful ſtar called Regulus or the Lion's heart.

This was not a real conjunction, ſince there was between all theſe ſtars the diſtance of ſeveral degrees. Theſe aſſemblages, reſpecting which aſtrologers formerly made ſo much noiſe, and which, according to them, were to be followed by dreadful cataſtrophes, have always paſſed over in a very tranquil manner, and have produced no other effect than that of covering with confuſion theſe ſilly prophets. They are alſo attended with no other benefit than that of affording to the aſtronomer an opportunity of obſerving ſeveral of the planets at the ſame time. C. Meſſier took advantage of this circumſtance to fix the reſpective ſituations of thoſe which were then in the neighbourhood of Regulus. The conjunction of ſeveral planets being a circumſtance very uncommon may ſerve alſo to fix the epoch of any event, and in ſuch an invariable manner that no change of æra or in the calendar can render it doubtful.

C. Prony read to the claſs a notice of ſome experiments made with an Engliſh rule, conſtructed according to the ſtandard of thoſe employed in the grand trigonometrical operation of general Roy, and brought to Paris by profeſſor Pictet of Geneva, and which was compared with the metre of the Inſtitute, and the toiſe called the Peruvian.

C. Flaguergues ſent ſome obſervations of the planets, and of different eclipſes.

Chemiſts were acquainted with two metallic combinations, which have the remarkable property of producing a violent detonation, when ſubjected to a ſlight compreſſion, or when expoſed to a ſlight degree of heat. Theſe combinations are fulminating gold and ſilver. Mr. Howard has lately made known a third, which poſſeſſes the ſame property, and to which

which he has given the name of *fulminating mercury*. It is attended, however, with this difference, that it is not produced under circumſtances ſimilar to thoſe under which gold and ſilver ſuſceptible of detonation are obtained. It is formed by the ebullition of nitrate of mercury with alcohol, and depoſits itſelf in a powder, the colour of which varies from white to a gray more or leſs dark. It was neceſſary to find in the analyſis of this ſubſtance an explanation of its production, and that of its reſemblance to fulminating gold and ſilver; and alſo of its differences from them. Mr. Howard concluded from his experiments, that it was compoſed of oxalic acid, oxide of mercury, and ethereal nitrous gas.

C. Berthollet has ſhown by his, that it contains no oxalic acid; but that it contains ammonia; that it forms in this reſpect a combination analogous to fulminating gold and ſilver; and that, conſequently, its effects ought to be explained in the ſame manner as thoſe of theſe compounds. Fulminating mercury differs from theſe two detonating metallic ſubſtances, by a portion of altered alcohol, which enters into the combination, and which, when decompoſed, produces carbonic acid. Fulminating mercury then is a triple combination, while fulminating gold and ſilver are only binary combinations of the oxides of theſe metals with ammonia.

C. Vauquelin read a memoir on the analyſis he made of a copper ore diſcovered in Derbyſhire, ſpecimens of which had been tranſmitted to the Council of Mines by Count de Beurnon. Vauquelin ſtates, that this ore is compoſed of 62 parts oxide of copper, 24 parts of arſenic acid, 8 parts arſeniate of iron, and 8 parts water of cryſtallization.

Vauquelin having been requeſted alſo by the Council of Mines to examine a mineral ſent to it by M. Karſtein of Berlin, under the name of arſeniated copper, aſcertained that this mineral is formed of 60 parts oxide of iron, 22 parts arſenic acid, and 18 parts water of cryſtalliſation; but that it contains not an atom of copper. Neither copper nor iron had been before found united to the arſenic acid; and this is the reaſon why the mineralogiſts had made no mention in their works of the arſeniate of iron or arſeniate of copper. The labours of C. Vauquelin have therefore enriched with two diſtinct ſpecies the catalogue of mineral ſubſtances already known *.

C. Vauquelin read alſo a memoir on a mineral found in the environs of Limoges, which C. Alluau, director of the porcelain manufactory in that city, had tranſmitted to him

* This is a *miſtake*. See Philoſophical Tranſ. 1801, part 1.—Edit.

under

under the name of *tin ore*. He has diſcovered that this ore is compoſed of 42 parts of the oxide of manganeſe, 27 phoſphoric acid, and 31 oxide of iron. He is of opinion that theſe three ſubſtances are intimately united, and form a kind of triple ſalt with a double baſe. This combination is abſolutely new to naturaliſts. No one had ſpoken of it before C. Vauquelin. It muſt hold a particular place, in the ſyſtems of mineralogy, either in the genus of iron or the genus of copper.

C. Gillet-Laumont read to the claſs an account of a mineral already known to naturaliſts, though they were too little acquainted with its ſituation in the earth. This ſubſtance, which contains a metal lately diſcovered by Vauquelin, is *chromated iron*. It had been ſeen in an inſulated maſs by C. Pontier, near Goſſin, in the department of the Var. But this mineralogiſt has ſince found it, in very great abundance, in the middle of a quarry of ſerpentine near the borders of the ſea, at a ſmall diſtance from the harbour of Cavalaire: and this diſcovery is valuable to the arts not only on account of the nature of the mineral, but alſo on account of the eaſe with which it can be procured in conſequence of the ſituation of the quarry.

C. Juſſieu read a notice of ſeveral kinds of Indian plants deſcribed by various authors, and which, in his opinion, ought all to be referred to the genus known under the name of *litſee*, the native country of which is China.

C. Delille, member of the Inſtitute of Cairo, tranſmitted to the claſs a memoir in which he gave an exact deſcription of the *doum* or palm of the Thebaid, which was before but imperfectly known.

The deſert plains by which Egypt is ſurrounded have not at all times been deſtitute of vegetable productions. At very remote periods they have been covered with trees, and particularly palms; and this may ſerve to account for the petrified trunk of a palm-tree found in the deſert near the iſthmus of Suez, and tranſmitted to the claſs by general Regnier, member of the Inſtitute of Egypt.

C. Regnier, his brother, added to this ſpecimen a manuſcript, written by himſelf, containing general conſiderations on the agriculture of Egypt, as well as the ameliorations of which it is ſuſceptible.

C. Ventenat preſented the 5th and 6th numbers of his deſcription of the new or little known plants cultivated in the garden of C. Cels.

C. Lamark preſented his *Annuaire Météorologique pour l'an* 10.

C. Van Mons, aſſociate, tranſmitted to the claſs the firſt volume

volume of a *Journal de Chimie*, which he has undertaken, and which is intended as a ſupplement to the *Annales de Chimie*, and other periodical works publiſhed on that ſcience.

He tranſmitted at the ſame time a tranſlation of an Italian work by Brugnatelli, profeſſor at Pavia, containing a chemical nomenclature by him, and a ſynonyme of the different modern chemical nomenclatures.

Lacepede tranſmitted to the claſs the third volume of *Hiſtoire Naturelle des Poiſſons*, 4to.

XIX. *Intelligence and Miſcellaneous Articles.* *February* 1802.

MINERALOGY.

THE *Journal de Phyſique* (Brumaire, an 10.) contains a notice of the diſcovery of corundum, at Cheſnut Hill, near Philadelphia, by Dr. Seybert. From a few experiments upon the moſt obvious properties of a ſpecimen received from the diſcoverer, made by Mr. Richard Phillips, to whom we are indebted for the preſent article, he is induced to believe that it is not corundum: it has for matrix decompoſed felſpar, accompanied by white felſpar and mica; only one cryſtal was contained in the ſpecimen, whoſe form was ſufficiently determinate for deſcription; it appeared to be a compreſſed hexaedral priſm, having one of its ſides interrupted in cryſtallizing; its colour inclining to green; its hardneſs ſufficient to give fire with ſteel, and to ſcratch glaſs; but it is itſelf eaſily ſcratched by corundum, and is not hard enough to mark the beryl; whereas the latter is readily ſcratched by corundum. It is eaſily reduced to powder. Its ſpecific gravity only 2,70, that of corundum being 3,87: the quantity was too ſmall to allow of accurate analyſis; upon examination it would probably be found to be quartz, which it nearly reſembled in fracture, deriving its colour from oxide of iron.

The following account of the diſcovery of a mine of native ſulphur in the Lower Pyrenees has juſt appeared in one of the French journals, in a letter from Dr. Thore. "Permit me to announce, through the medium of your journal, the diſcovery made ſome time ago of a mine of native ſulphur, by a proprietor in the commune of Saint Boué, department of the Lower Pyrenees. Accident, the grand author of diſcoveries, was the author of the one in queſtion.

"Some works undertaken for repairing a water-mill laid open at firſt a quarry of gypſum, and ſoon after the mine to which I here allude. When informed of it, I repaired to the ſpot to obtain the neceſſary information reſpecting it;

I at

I at the ſame time wrote to my friend Darracq, preparer general at the School of Mines, to requeſt his opinion of it. He ſent me notice that he would accompany me to viſit it; which we accordingly did, and I can verify the diſcovery, till we give a detailed memoir on the advantages or inconveniences likely to reſult from it.

"In the mean time, I think it my duty to ſay, 1ſt, That it is ſituated in a very deep ravine, and to the ſouth of a redoubt, known under the name of the redoubt of Hillot, which commands it by about 150 toiſes at leaſt.

"2d, That the ſulphur preſents itſelf in two very diſtinct ſtates. In the firſt, it has for matrix ſometimes earth of a gray aſh colour, which evidently ferments with acids without being converted into lime, and ſometimes gypſum, at the ſide of which the ſulphur is found. In both theſe caſes it is of a beautiful lemon yellow colour, and tranſparent as glaſs; ſometimes it is detached in blocks of ſeveral pounds, or enveloped in a foliaceous mixture, which we have not yet been able to determine. In the laſt caſe, the ſulphur is of the colour of colophonium, and appears to have undergone an evident degree of fuſion, whatever may have been the agent that occaſioned it.

"3d, If the matrix, which is very hard, be divided, it emits a very ſenſible bituminous odour, and the fractures ſhow geodes of different ſorts. Some contain only petroleum, and others very curious cryſtallizations.

"4th, This mine, during great heats, exhales ſo ſtrong an odour, that the proprietor was obliged to cover it with ſeveral feet of earth to ſhelter himſelf from it."

SUGAR FROM THE BEET-ROOT.

Doctor Achard has publiſhed, at Berlin, the reſults of a ſecond trial, made on a large ſcale, to extract ſugar from the beet-root, under the direction of a commiſſion appointed for that purpoſe by his Pruſſian majeſty. By theſe reſults it appears that 1500 quintals of beet-root gave 5952 pounds of raw ſugar; 450 quintals of refuſe, and 100 ounces of ſyrup. Thirty quintals of beet-root, cultivated according to the proceſs of Achard, gave each ſix pounds three ounces of raw ſugar. The refuſe may be employed as coffee, or to diſtill ſpirit; and is more profitable for feeding cattle than beets themſelves. The raw ſugar may be refined for every purpoſe whatever. According to a calculation made by the commiſſion charged to examine this diſcovery, it will produce to Pruſſia an annual ſaving, or rather an advantage, of two millions and a half of ſix-dollars.

MUSIC.

MUSIC.

M. Horſtig, counſellor of the conſiſtory of Minden, has invented an ingenious method of marking the muſic of an air or ſong without employing the common muſical notes. Inſtead of notes he makes uſe of figures, which expreſs the notes equally well: thus 1 always ſignifies the tone of the piece, whatever it may be; 2, the ſecond; 3, the third; 4, the fourth, &c.: but if the piece is played or ſung in *fa*, the cipher 1 denotes *fa*, 3 *la*, &c. The following is given as an example:

Cœurs ſenſibles, cœurs fidèles,
Qui blâmez l'amour leger.

The muſic of theſe lines is thus repreſented:

5 3 1 3 2 4 4 3

3 4 5 5 6 5 4 3 3 2

If the piece then is played in *fa*, we have

re ſi ſol ſi la ut ut ſi

If any other tone be aſſumed, the ſame air will always be preſented.

VACCINE INOCULATION.

We are informed by a member of the Vaccine Inſtitution, that cow-pock matter ſent upon glaſs, from Milan, by M. Saccho, taken from the Milaneſe cows, as communicated to Dr. Pearſon, has been employed on ſeveral patients at the Inſtitution; on all of which it has produced preciſely the ſame diſeaſe as that from the Engliſh.

On the 23d inſt. Mr. Blair began a courſe of popular lectures on anatomy and the animal œconomy: wherein the ſtructure and functions of the human body are to be familiarly explained and illuſtrated; for the information of ſcientific perſons, amateurs of natural hiſtory, and ſtudents in the liberal arts.

The plan of the courſe is as follows:

Component parts of the body.—Bones, cartilages, and ligaments.—Muſcles and muſcular action.—The integuments and membranes.—Brain, nerves, and ſenſation.—The heart and vaſcular ſyſtem.—Blood, circulation, and abſorption.—The glands, ſecretions, and excretions.—Reſpiration and animal heat.—Digeſtion, nutrition, and growth.—Utero-geſtation, and parturition.—The eye, and phænomena of viſion.—Functions of the ear, noſe, and mouth.—Phyſiognomy, beauty, and the paſſions.

XX. *An Examination of* C. CLOUET'S *new Process for making Cast Steel from Bar Iron by means of the Decomposition of Carbonic Acid, continued*. By* DAVID MUSHET, *Esq. of the Calder Iron Works.*

HOWEVER conclusive the experiments communicated in my last appeared, in opposition to the theory adopted by the Parisian chemists,—that the decomposition of carbonic acid afforded the steely principle to iron, I deemed the examination imperfect, unless experiments were made with various carbonates, and particularly with those foreign to this country. With a view therefore to give every degree of justice and candour to the investigation, the experiments which follow were carefully and accurately performed.

Exp. VII. Fusion of the same fragments of Swedish iron with equal portions of Italian marble and Stourbridge old clay pot.

	Grains.
Pieces of Swedish iron - -	916
White Italian marble, 1 3d 305·3 grs.	
Stourbridge old clay pot, 1-3d 305·3 grs.	
This is the exact proportion prescribed by Clouet for the formation of his best cast steel. From the mixture, I obtained in 40 minutes a very perfect fusion. The metallic button possessed a dense smooth surface, free from pits and honeycombs. It weighed -	897
Lost in fusion	19

equal to $\frac{1}{48\frac{3}{7\,6}}$th part of the original weight of the iron. The incumbent glass was very perfect, and of a smooth dense surface. Its colour was light bottle green, some shades purer than the glass of experiment No. 1. afforded with Balgrochan limestone.

The button now obtained was cut, and broken across an anvil. Its fracture was regularly granulated, of a dark blue grain, very prominent, but pervaded with a dull lead colour betwixt the crystals. One-half of this button was drawn into a small drilling tool, and hardened with a degree of heat beyond that of common steel. The file, at first, met with a slight resistance; but a few strokes removed the partial hardness, and an uncommon soft iron succeeded. This drill was introduced below a drilling frame loaded with only 7 lbs. upon the end of the lever. The brace was not turned half

* For the former part of this Examination see p. 27 of our last Number.

round before the point had ſet up all round like lead. The ſame piece was afterwards ſubjected to ſeveral trials of hardening, bending, and filing, in all which it manifeſted properties peculiar to the ſofteſt malleable iron. The denſity conveyed to the metal by fuſion, ſeems to form an univerſal characteriſtic feature of this ſpecies of metal. When the file is applied to the melted maſs, it meets with a much greater degree of reſiſtance than from common malleable iron; but this exiſts no longer after being forged, and the file then acts with the greateſt poſſible effect.

	Grains.
Exp. VIII. Pieces of Swediſh iron weighing	1090
Italian marble - 2180 grains,	
Stourbridge clay, old pot 2180 grains.	
This proportion is ſix to one of that preſcribed by C. Clouet, and from which gray caſt iron ſhould have been obtained. In 45 minutes I deemed the fuſion complete, and withdrew the crucible. When cold, I found a very perfect metallic button reſting beneath a great thickneſs of glaſs. The external appearance, in every point, reſembled that of No. 1.	
It weighed - - -	1070
Loſt in fuſion	20

equal to $\frac{1}{54\frac{1}{2}}$ part the original weight of iron. The glaſs in this experiment was ſeveral ſhades lighter than that obtained in No. 7. The reaſon for this ſeems obvious, as ſeven times the quantity of glaſs poſſeſſed only a quantity of colouring principle ſimilar to the former, if by this be underſtood the ſpecific quantity of iron loſt in fuſion. The fracture of this button was preciſely that of No. 7. One-half of it was drawn into a ſmall cutting chiſel, and hardened at a pretty bright red heat. It ſhaled a little, and felt, for a few ſtrokes, perceptibly harder under the file. Upon applying the chiſel to cut a piece of newly hammered Swediſh iron, it made a conſiderable impreſſion with a moderate blow from a 3 lb. hammer; but the whole edge was flattened, and at a ſecond blow the edge ſtaved completely, and meaſured nearly 1-8th part of an inch acroſs.

A fragment of the ſame experiment was ſubjected to a variety of teſts, in none of which the moſt diſtant ſymptom of ſteel was manifeſt: on the contrary, in point of ſtrength and durability it was found inferior to pieces of the original bar.

	Grains
Exp. IX. Swediſh iron, in pieces -	1099
Italian marble - 1099 grains.	

This

Grains.

Brought over 1099

This mixture was expoſed to a violent degree of heat, and in one hour I judged the fuſion complete. The button obtained was finely poliſhed upon its upper ſurface, and remarkably denſe. It weighed - 1059

Loſt in fuſion 40

equal to $\frac{1}{27\frac{4}{10}}$th part of the original weight of the iron. The fracture of the button now obtained was of a ſmaller grain than the former, equally regular but leſs brilliant. It drew into ſhape equally well under ſimilar circumſtances with the former, and exhibited the very ſame appearances.

One-half of this button was forged into a point, and hardened at a high heat for common ſteel. I endeavoured to pierce a piece of thick rolled plate iron; but at the firſt blow the point bent upwards, throwing a white ſhale and leaving the ſurface like tin.

The glaſs obtained by the fuſion of lime alone in this experiment was very different from that in Nos. 7 and 8. It poſſeſſed a rich graſſy green colour, free from air bubbles and very tranſparent. The extra loſs of metal in this experiment beyond that in Nos. 7 and 8. may be accounted for from the infuſibility of carbonate without the uſual addition of clay. I have repeatedly found that lime alone, when pure, gradually contracts its bulk, and remains unfuſed a conſiderable time after the iron has entered into fuſion. This naturally expoſes the latter to a greater degree of oxidation than when covered by a fuſible glaſs during the greateſt part of the expoſure.

The following experiments were performed with Kilkenny blue marble:

Grains.

Exp. X. Pieces of Swediſh iron weighing - 1196

1-3d of Kilkenny marble, or 398 grains.

1-3d of Stourbridge clay pot, or 398 grains.

There reſulted from this mixture, after half an hour's expoſure, a very fine fuſion accompanied by a very perfect metallic button of - - 1173

Loſt in fuſion 23

equal to $\frac{1}{52\frac{8}{10}}$th part of the firſt weight of the iron. The colour of the glaſs now formed was ſome ſhades lighter than in experiment No. 7. The green was ſlightly tinged with an amber colour, which difference, moſt probably, aroſe from the nature of the lime.

The fracture of this button differed little from Nos. 7 and 8, and in forging no difference could be detected, which implied a marked diſtinction as to quality. Grains.

Exp. XI. Swediſh iron, in pieces - 1012
Kilkenny marble - 2024 grains,
Stourbridge clay, old pot 2024 grains.

This proportion of earths is 6 to 1 of that preſcribed by C. Clouet. The mixture was expoſed for nearly 50 minutes, when I judged from the degree of heat that the fuſion was complete. When cold, I found a large maſs of glaſs reſting upon a very perfect ſmooth-ſkinned metallic button, which was found to weigh 989

Loſt in fuſion 23

equal to $\frac{1}{44}$th part of the original weight. The fracture was ſo like that of the former, that they were not eaſily diſtinguiſhable from each other. The ſame ſimilarity in point of quality, when forged and put to various teſts, was alſo evident.

The colour of the glaſs was as much alike to No. 10 as were the qualities of the reſpective metallic products. Grains.

Exp. XII. Swediſh iron - - 878
Kilkenny marble 1756 grains.

From the purity of the Kilkenny marble I judged it requiſite to expoſe this mixture to a very high degree of heat, which continued nearly one hour. I found a perfect fuſion of the lime, and a very fine button of metal. The latter weighed - - 828

Loſt in fuſion 50

equal to $\frac{1}{17\frac{20}{50}}$th part of the original weight. The form, appearance, and denſity of this button exactly correſponded with that of No. 9, when Italian marble was fuſed along with iron. Its fracture was in moſt reſpects ſimilar, and the ſubſequent trials proved that its quality was alſo the ſame. The loſs in metal was greater in this experiment than in No. 9, probably ariſing from the iron being longer expoſed under a high temperature before the carbonate, by reaſon of exceſſive purity, entered into fuſion. The glaſs produced by the fuſion of this marble in contact with iron was of a black colour, and poſſeſſed of no great degree of luſtre in fracture. When pounded ſmall, it aſſumed a ſhade of dirty green, differing materially from any of the former.

I have avoided entering into a detail of the various appearances which theſe three laſt products aſſumed in forging. Their

Their ſimilarity and identity with thoſe deſcribed in Nos. 7, 8, and 9, rendered ſuch a deſcription unneceſſary. Suffice it to ſay, they were applied in the ſame ſhapes to the ſame ſpecies of labour, and found equally inapplicable to the purpoſes for which they were intended.

Neither in the experiments recorded in this paper, nor in thoſe which accompanied the former communications, was there found any product reſembling ſteel, or poſſeſſed of any of thoſe properties which iron derives from its combination with carbon: on the contrary, the reſulting product in all of them was found debaſed, in point of ſtrength and elaſticity, below the ſtandard of common malleable iron. The uniform quality of the various metallic maſſes, though fuſed in contact with widely different proportions of earths, leaves little room to doubt that the latter are entirely neutral; nor does it appear, upon a review of the experiments performed with a mixture of carbonate and argil, that the loſs of metal is dependent upon the quantity of earths fuſed in contact with it, as the following table will ſhow: the ſame review exhibits a triple loſs of metal in thoſe experiments wherein the carbonate of lime was fuſed alone with the iron.

Experiment No. 1. loſs was one 73d part.
2. - - 70th
3. - - 79th
4. - - 44th
5. - - 40th
7. - - 48th
8. - - 54th
10. - - 53d nearly
11. - - 44th

Average loſs in fuſion = one 56th part.

The three firſt fuſions ſeem to have experienced a ſmaller loſs in metal than the ſubſequent ones. This I am apt to attribute to a comparatively imperfect fuſion, as the buttons obtained in Nos. 1, 2, and 3, were conſiderably pitted, and every way leſs denſe than thoſe afterwards produced, when I had learnt to regulate the neceſſary heat to a certainty.

Experiment No. 6. loſs one 17th part
9. - 27th
12. - 17th

Average loſs in fuſion one 20$\frac{1}{3}$d part.

It appears alſo that the quantity of carbonate uſed, and the loſs of iron ſuſtained, were not analogous to each other. To verify this, No. 6 and 12 may be compared.

In the courſe of making the ſeries of experiments from which the foregoing are ſelected, I had occaſion to remark that the mixture of argil and carbonate, when moderate proportions were uſed, entered into fuſion in 10 to 15 minutes from the period of being introduced into the furnace, and from 15 to 20 minutes before the iron melted. A few crucibles were withdrawn at this early ſtage of the proceſs; the glaſs was found perfectly pure and tranſparent. The pieces of ron were more or leſs welded together, but poſſeſſed their original ſhape and quality. And though it may ſeem ſomewhat foreign to the preſent ſubject, I ſhall further remark, that ſo long as the pieces of iron remained unfuſed or fuſing, a continual bubbling took place upon the ſurface of the liquid glaſs reſembling the diſcharge of an elaſtic body. I ſoon learned to prejudge the degree of perfection in the fuſion by the ſurface of the glaſs. In imperfect reductions this was completely covered with various ſized bubbles, many of which were concentric: on the contrary, when the metallic button was found poſſeſſed of ſmoothneſs and uniform denſity, the ſurface of the glaſs was either entirely ſmooth, or at moſt preſented one ſolitary bubble exactly in the centre, through which it is preſumable, at the time of its fixing, had been diſcharging the laſt portions of the gaſeous ſubſtance formerly alluded to.

In purſuing the inveſtigation of this ſubject, I had prepared a variety of other experiments. The fuſion of the ſame Swediſh iron with deacidified carbonate and argil, in various degrees of mixture, and with the former alone, formed a diſtinct head. The ſimilarity of the reſults, excepting an alteration in the colour of the glaſſes, renders theſe experiments no longer intereſting. Others, however, perſormed with Britiſh cold ſhort iron, made in the ſtamping proceſs, are more worthy of notice.

Various weights of iron were fuſed with mixtures of argil and carbonate, and with the latter alone. In every caſe, a much greater quantity of metal diſappeared than in any of the former experiments, and a ſimilar difference in the appearance of the glaſs.

1ſt, In two experiments performed with cold ſhort iron and an equal mixture of 1-3d each of Kilkenny marble and Stourbridge clay (old pot) in the one, and double the weights of the iron of each in the other, the loſs of metal was $\frac{1}{11}$th and $\frac{1}{16}$th: average, $\frac{1}{13\frac{1}{2}}$. The accompanying glaſs was poſſeſſed of an uncommon degree of luſtre and denſity. Its fracture preſented a poliſh and colour ſuperior to black marble: very

very thin ſplinters, however, diſplayed a rich amber-green colour.

2d, 1000 grains of Kilkenny marble and 500 grains of cold ſhort iron were fuſed, with a loſs of $\frac{1}{10}$th of metal. The glaſs was of a deep graſs-green colour.

3d, Equal mixtures of 1-3d each of Italian marble and clay pot fuſed with 1000 grains cold ſhort iron, a loſs of $\frac{1}{15\frac{4}{10}}$th part was ſuſtained.

4th, 2000 grains of Italian marble and 1000 grains of cold ſhort iron were fuſed together: a very black, ſmooth, ſhining glaſs was obtained, and a loſs of 1-9th of iron was experienced.

5th, I welded ſeveral pieces of this cold ſhort iron; drew it into a bar; cut the bar acroſs; welded the pieces, and again drew it into ſhape. The iron ſeemed conſiderably improved. 1000 grains of this iron were fuſed with 666 grains of an equal mixture of Italian marble and Stourbridge clay pot: the loſs in metal only amounted to $\frac{1}{25\frac{1}{2}}$ part of the original weight of the iron.

The foregoing experiments are, I conceive, in point of variety and accuracy, ſufficient to demonſtrate, that in no proportion, either alone or in mixture, does carbonate of lime, by the decompoſition of its acid, or otherwiſe, communicate carbonaceous matter to iron fuſed in contact with it. In ſeeking, therefore, for an explanation of the phænomena which have ſo completely deceived the Pariſian chemiſts, the two following reaſons occur as being moſt probable. The crucibles uſed may have contained a portion of carbonaceous matter, or the reporters have been deceived by the ignorance or intention of their artiſts. If the latter has been the caſe, then the concluding paragraph of my communication in January will apply with peculiar force: if the former, the following experiments will ſhow how eaſily the deception might have taken place.

1ſt Experiment with black lead crucibles. Grains.

Swediſh iron - - - 6959

Calcined carbonate of lime 1750 grains,
Stourbridge clay pot - 1750 grains.

This mixture was expoſed to a degree of heat capable of fuſing a ſmall portion of caſt ſteel, and in an hour I found the contents completely reduced. When the crucible was withdrawn, and cooled, I found an ingot of metal; the upper ſurface of which was beautifully

	Grains.
Brought over	6959

coloured, and cryſtallized in the moſt perfect radii, feathered tranſverſely. The under ſurface was conſiderably honeycombed, and the concaves poſſeſſed minutely cryſtallized interiors. The maſs weighed 7058

Gained in fuſion 99

equal to $\frac{1}{70}$th part nearly of the original weight of the iron employed.

The glaſs in this experiment was milky blue inclining to azure; denſe, and very tranſparent.

The ingot was cut into three pieces, each of which was drawn into a neat bar. The caution neceſſary was great, as the product upon the firſt operation diſplayed an intimate connection with ſteel, and proved to be ſteel very highly ſaturated with carbon. A razor and ſeveral penknives were made from this reſult, all of which hardened to exceſs with a low ſhade of red.

A fair inference from this experiment is, that the carburet of iron which entered into the compoſition of the crucible, here united in part with the iron, and formed very highly ſaturated ſteel. The precaution uſed by introducing deacidified lime leaves no doubt upon the ſubject of the carbonic acid.

2d Experiment with black lead crucibles. Grains.

In this experiment I uſed of the ſame iron - 3514

Of calcined lime - 440 grains,

Old clay pot - 440 grains.

A very beautiful button of ſteel was obtained in this fuſion, the cryſtallized ſurface of which was ſo prominent as to leave a very perfect impreſſion upon the under ſurface of the incumbent glaſs. The weight was found to be - - - 3567

Gained in weight 53

equal to $\frac{1}{66\frac{3}{10}}$th part of the original weight of the iron. The quality of the ſteel now obtained was ſofter than that produced in laſt experiment, and more ſuitable for the ordinary purpoſes to which caſt ſteel is applied.

3d Experiment with black lead crucibles. Grains.

Swediſh iron - - - 3630

Calcined lime - 220 grains,

Stourbridge clay pot - 220 grains.

From this mixture another elegant button of metal

covered

	Grains.
Brought over	3630
covered by a thin layer of glaſs was obtained, which weighed - - -	3623
Loſt in fuſion	7

equal to $\frac{1}{518\frac{4}{10}}$th part of the weight of the iron employed.

This ſteel was, in point of quality, ſofter than any of the former; and from this circumſtance I inferred that the earths ſo far contributed to the ſteelification of the iron by fuſing upon the ſurface of the metal, and preventing the eſcape of the carbon. It appeared alſo further adducible, that if the iron was preſented with a ſufficient doſe of carbon to make allowance for waſte, ſteel ſhould be formed without the addition of any portion of earth.

4th Experiment will juſtify the firſt inference. Grains.

Into a black lead crucible was introduced - 3710 of the iron employed in former experiments. A tight lid was fitted to the crucible, and the iron expoſed to as high a heat as I judged the crucible capable of ſuſtaining. This continued, from firſt to laſt, for 38 minutes, when the crucible was withdrawn. When cold, I found that the iron had ſunk to the bottom, forming an imperfect ingot of ſteel of a proper degree of ſaturation of carbon at the under part, but almoſt of the nature of malleable iron towards the upper ſurface. It weighed - - - 3692

Loſt in fuſion	18

equal to $\frac{1}{206}$th part of the weight of the iron.

The latter inference was juſtified in the courſe of ſome hundreds of experiments with various carbonaceous ſubſtances and iron, the ultimate reſult of which proved that excellent caſt ſteel could be formed in crucibles deſtitute of carbonaceous mixture by the ſingle addition of carbon.

XXI. *Experiment made with a Galvanic Belt, or Chain, by Mr.* RICHARD TEED, *Jeweller, Lancaſter Court, Strand.*

To Mr. Tilloch.

SIR, Feb. 4, 1802.

I HAVE not obſerved, in any publication on galvanic electricity, that material benefit has been as yet attained by this new diſcovery, and therefore I ſhall beg leave, through

the medium of your valuable Magazine, to ſubmit the following experiment to the conſideration of your readers, with the hope that others will be induced to try it, and alſo that it may afford relief to ſome labouring under ſimilar complaints.

For a conſiderable time paſt I have been much troubled with a conſtant pain in the ſmall of my back and loins, and, although it reſembled the lumbago, it was ſcarce ever ſo violent; but I always felt it moſt after ſitting long in a reclined or writing poſture; ſo that, if I aroſe ſuddenly, it was with much difficulty. This complaint continued for eight or ten months, and lately with increaſing inconvenience. The idea of a galvanic belt or chain, compoſed of zinc and copper, had often occurred to me, and I waited only in hopes of hearing it had been applied in ſimilar caſes: but the experiments of ſcientific men taking a different direction, I made a belt conſiſting of fifteen ſmall ſquare plates of zinc, and connected each with two links of plated copper wire, faſtening the two ends with a common hook and eye, ſo that there was a perfect chain or circle of metal round my body, and by that means no interruption of the electric fluid could take place. I alſo covered about three parts of the chain with leather, leaving the remainder to come into contact with the part where I felt the moſt pain. I had not worn this belt twelve hours before I found ſenſible relief, and the pain gradually left me. In three weeks I had not the leaſt return, and after wearing the belt three months I concluded it had anſwered all my expectations. But, to put the experiment beyond the poſſibility of doubt, I diſcontinued it, and had no pain whatever in my back for two months, when at times I perceived the ſame pain return. I again had recourſe to the belt, and am now wearing it, which, as at firſt, has removed all pains in that part, and I feel no inconvenience whatever.

A ſhort time after uſing the belt I obſerved a conſiderable oxidation on the zinc, which, I ſuppoſe, was occaſioned by the perſpiring matter from my ſkin, and which, I conclude, was the medium or exciting agent, as is the caſe with the diluted nitric acid in the galvanic pile. By ſcraping off the oxide, which I conſtantly did once in three or four days, I believe the effect became greater.

In giving you this plain but circumſtantial account, I truſt, a diſcovery ſo valuable will not be ſlighted, but that unprejudiced perſons will alſo make the trial; and I particularly recommend it to the attention of thoſe who have complaints of the ſame kind.

XXII. *In-*

XXII. *Inquiries into Coloured Light, by a Collation of the Experiments and Obſervations made by Sir Iſaac Newton on that Subject; together with ſome additional ones. By Governor* POWNALL.

[Concluded from p. 49.]

THE green priſmatic tint is ſuppoſed to be homogeneous, becauſe conſiſting of angles of refrangibility in the yellow and in the blue ſo exactly connected in degree at the point where they coaleſce, that no priſm nor any ſucceſſion of priſms have as yet decompoſed it. It is, however, in various other inſtances decompoſed; in the circles of colours ariſing from the refractions of light paſſing between two lenſes, as alſo in the rings of colours exhibited on bubbles of water. In theſe inſtances its conſtituent parts, which combined form the compound green, emerge as yellow on one ſide and blue on the other.

Sir Iſaac Newton, in Obſerv. iv. part 1. book 2. marking the order of colours in the circles produced by the refraction of light between two lenſes, ſays, "the green was very copious and lively, inclining on one ſide to blue, on the other to yellow:" and again, in part ii. book 2. p. 202. he ſays, "the green exhibited is principally conſtituted of *original green*, but not without a mixture of blue and yellow:" again, in part i. book 2. p. 190. he ſays, "then after a lively orange followed an *intenſe bright* and copious yellow, which was alſo the beſt of all the yellows; this changed firſt to a greeniſh yellow, then to a greeniſh blue; but the green between the yellow and the blue was very little and dilute, ſeeming rather a *greeniſh white* than a green: the blue which ſucceeded became very good." In this obſervation made on the rings of colours on bubbles, notice the increaſing intenſeneſs of the light; alſo ſucceſſive gradations; alſo the gradual proceſs of the compoſition of green and of its decompoſition, which is given in the next obſervation, p. 189: "After the red ſucceeded little or no yellow, but a copious green, which at firſt inclined a little to yellow, then became a pretty briſk and good *willow-green*, and afterwards changed to a blueiſh colour; but there ſucceeded neither blue nor violet." In the firſt inſtance, the very little and dilute greeniſh white, as well as the willow-green in the ſecond, was the dominancy of an intervening white, as will come out hereafter; and in both inſtances the yellowiſh green and blueiſh green mark the compound, partaking on each ſide of the yellow or of the blue,

blue, as the one or the other predominated in the compoſition.

But to put this matter out of all doubt, the *fact* comes out on experiment, that when the gradations of the colour take ſuch an arrangement that there is no interſection of the yellow orbicular images with thoſe of the blue, and ſo no coaleſcence of theſe colours, *there is no green.*

Obſerv. iv. part i. book ii. p. 174. Sir Iſaac Newton ſays, "the colours reckoned in their order from the centre were, black, blue, *white*, yellow, red; here the blue and yellow not interſecting or coaleſcing, but ſeparated by an intervening white, there was *no green* in this gradation of colours: if they had approached ſomewhat near to an interſection of their circles, there might have been, as above, a whitiſh or willow-green bordered with yellowiſh and blueiſh green. The ſame fact is ſtated in Obſervation ix. p. 181: the order of this gradation was, "violet, blue, white, yellow, red." Here again an intervening ſpace of white excludes the exiſtence of green. The ſame fact of an *intervening white* between the yellows and the blues, excluding the green, comes out in ſome experiments and obſervations made by G. W. J.* in addition to thoſe made by Sir Iſaac Newton, where, in p. 99. he ſtates, that "diluted purple and blue hues appear completing, with the external yellow and red, and an *intervening white*, theſe formations of theſe fringes." I will venture to add, in confirmation of all this, one amongſt ſeveral obſervations which I made on theſe facts. Viewing, through a priſm, the ſolar light as it came through a ſaſh window, and taking the ſpectrum in the line of refraction as it came from above, the following circumſtances appeared:—The frames of the ſaſh were dark, and gradations of colours proceeded from them. The gradations of blue on the upper ſide; thoſe of the red, orange, yellow, from the lower ſide. The gradations proceeded from the upper ſide in this order—deep blue, and, in ſucceſſion, the ſeveral lighter tints or hues of blue till they vaniſhed in the ſolar light. From the under part they proceeded in this order—deep red, then a brighter, going into orange, and the orange ſucceſſively brightening into yellow, which melted off into the ſolar light: no gradations of blue ſucceeded either order, becauſe there was no going off of light into ſhade when the ſpectrum was thus tranſparent to the ſolar light; and as there was no blue to intermix with the laſt yellow, there was *no green.* In proof that this is the reaſon, it may be obſerved, that if the light itſelf

* A very ingenious work publiſhed by Cadell 1799.

through

through which the ſpectrum is viewed, decreaſes, in any conſiderable degree of its brightneſs, there will come on blueiſh hues, and a very faint intervening greeniſh one. To try this circumſtance more decidedly, I threw up the ſaſh, and made an aſſiſtant hold a broad rule, ſo that I might view the ſpectrum as it proceeded from the two edges in the ſame manner as from the frames of the ſaſh: I then directed my aſſiſtant to lower the rule down to the bottom of the window, till the yellow in the under gradations interſected the bottom. The yellow of this poſition not going off into tranſparent light, but into ſhade, the blue hues appeared in very decided tints through all their gradations; and where blue, ſucceeding to the yellow, interſected it, a decided green intervened.

Although no common eye, unpractiſed in the uſe and arrangement of colours and their various tints, can diſcover in the ſpaces of the priſmatic colours called homogeneous, when thrown on an opaque ſurface, any gradations of tints; yet when theſe colours are viewed by the priſm through an open light ſo as to be tranſparent, the gradation of ſucceſſive tints in each homogeneous ſpace becomes evidently obſervable to the moſt common eye. The obſerver may diſcern the fact, that no one of thoſe priſmatic colours which are called homogeneous is abſolutely ſo, but that, as according to the theorem ſtated by Sir Iſaac Newton himſelf, they conſiſt of a ſucceſſive ſeries of innumerable circular or orbicular images of the ſun; the tints of the commonly called homogeneous colour, red, vary according to the innumerable ſucceeding angles of refraction, heightening gradually towards *orange*, and going gradatim into the orange; and that the orange heightens in the ſame gradual ſucceſſion of tints going into yellow, and the yellow, in like manner, into pure light: alſo that the blues of the other gradation of colours are an indiſcriminate ſucceſſion of tints or hues.

Conformably to what is here ſtated of the heightening of the gradations of the colours at one end of the ſpectrum into light; of the blues going off, by a like gradation at the other end, into ſhade or dark; Sir Iſaac Newton (Optics, book i. prop. v. exper. 16. p. 85.) ſtates, that "the moſt luminous of the priſmatic colours are the orange and the yellow; next the green; and that blue is a faint and dark colour, and the indigo and violet the weakeſt and darkeſt of all colours."

Now from a more deciſive examination of the fact by actual experiment we ſhall be led to ſee demonſtrably that theſe apparent hues, which are called blue colours, are *not colour*, but a mere modification of light going off into ſhade, *a partial and ſucceſſive deprivation of light.*

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In the firſt place, it is known by Sir Iſaac Newton's experiment, (Optics, book ii. part iii. prop. 7. p. 236.) "that blacks are inclined to a blueiſh colour may be ſeen from illuminating white paper by light reflected from black ſubſtances, for the paper will appear of a *blueiſh white.*"

In conſidering this obſervation on a fact as made by Sir Iſaac Newton, the method to try the effect of throwing a partial portion of light on a ſhade occurred to me. I darkened a room partially, letting in a beam of light of five or ſix inches breadth by opening one of the ſhutters. I then threw the ſhade of a ruler, by the intercepted light of a candle, on a ſheet of paper: I then held this paper, with this ſhade ſo formed on it, in the beam of light. The effect of this portion of daylight thus thrown on the ſhade of the ruler was, that the ſhade exhibited a pale but decided blue. As I removed this paper, with this ſhade of the ruler thus thrown on it, further back from the entrance of the light, but ſtill in the ſame beam, this blue, without the alteration of any one circumſtance but a further deprivation of light, became of a ſtronger, more decided, deeper blue; and ſo on gradually as I removed this paper and ſhade ſtill further back, making a further deprivation of light of a deeper and deeper but decided blue till it went off into a black. This ſhade, ſuffering this gradual deprivation of light, exhibited all the gradations called blue which are ſeen in the priſmatic ſpectrum. Here, then, the fact ſtands aſcertained, that blue is the effect of a partial light, and that a gradual deprivation of light gives all the gradations of blue as they go off into dark.

Mountains and hills, and even unelevated land, in the horizon exhibit, by the going off or deprivation of light, blue tints in the moſt diſtant, though ſuch are not the natural tints of theſe objects; and as theſe objects ſuffer more and greater deprivations of light in the evening, or from any other incidental circumſtance, theſe hues go through all the gradations of blue. This any one may ſee every day; and the painter, if he is an artiſt, knows how to copy this phænomenon in the arrangement of his colouring.

Another circumſtance in the landſcape of nature is ſtill further to be obſerved. As the ſhade of evening comes on, the blue in the diſtant horizon not only becomes deeper, but, when the departing light ſkirts the line of the horizon, this blue, without any change in the ſtate of the refractions, or of the actual colour of the objects, becomes a ſtrong vivid purple. The dark blue of the clouds juſt above this ſkirting of departing light, changes in like manner. The rays of the reddiſh-yellow light and thoſe of this blue coming in coincidence

cidence at the ſame time to the eye, cauſe that ſenſation in the optic nerve.

The ſky alſo, the leſs that clouds and reflecting vapours are mixed with it, abſorbs the light, and gives out only this blue hue, which is a reflection deprived in part of light *: the more clear the ſky, the deeper the blue: in the higher regions of the atmoſphere this blue becomes of a deep hue going off into dark.

This circumſtance takes place and prevails in the claro-obſcuro of all figures. A true artiſt knows, that if he would copy the *real fact* in nature, he muſt tint the going off of light not with gray and black but with gradations of blues, and give a faint ſuffuſion of green in the firſt going off. Zincke, the famous painter in enamel, underſtood and copied this fact: he worked an almoſt inſenſible green into the blues of the firſt going off of his lights.

If one may then aſſume the fact here grounded on the experiments and obſervations of Sir Iſaac Newton, with addition of ſome further ones on the caſe as exiſting in nature, that the gradations of blue are mere effects of the privation of light as it ſinks into ſhade or darkneſs; and then, from the teſtimony of the viſual ſenſes, which are the only teſt in the experiments above related, that the gradations of the red even in the priſmatic ſpace, wherein it is ſuppoſed to be homogeneous, of the orange likewiſe, and ſo of the yellow, are only gradual and ſucceſſive tints of the *one primary colour, red*, heightening, by an innumerable and infinite ſeries of angles of refraction, into a more and more brilliant approximation of pure light, as they purify from the ſuffuſion of colour which they had received in their combination with ſolar or terreſtrial gaſes and vapours; it will, on this aſſumption thus grounded on facts, ſtand demonſtrably proved (although I do not venture to announce it but with every diffidence) *that there are not ſeven, nor three, nor two, but* ONLY ONE PRIMARY COLOUR—*a compound baſis of all the tints of red, orange, and yellow: that green, as an actual and uncompounded primary colour, has no exiſtence: and that the blues are only hues ariſing from a partial deprivation of light as it goes off into darkneſs.*

That all the phænomena of colours produced by the re-

* This circumſtance, alſo, makes the difference of the blue of diſtant parts of the landſcape as reflected from valleys and lower parts, or from the higher: the deprivation of light, by its being abſorbed by the clearer air in the latter caſe, gives a deeper blue than that which is reflected from the air hanging over the lower parts. This a true artiſt, who paints from nature, knows.

fraction

fraction of prisms or of lenses, or by thin plates of air or water between lenses made to approach each other, or by the rings on bubbles, or by reflections or inflections of light, may be explained from the facts contained in the above proposition.

All the experiments, observations, and reasoning above, respect only *coloured light*. I dare not venture, on theory, to assert any thing as to the existence of an elementary, pure, uncoloured, uncompounded white light; yet that some such elementary substance exists, combined with one or more of the principles of our atmosphere, and also in combustible and incombustible, and perhaps animal and vegetable substances, seems to be probable; for it can be separated from such in various operations of nature and art.

However, although light may principally exist, combined with caloric and other substances, in a gaseous form, yet it may, and it is very generally admitted, that it does also exist, independent of caloric, in solid bodies and fluids of various kinds; and heat, it cannot be denied, exists sensibly in both combustible and incombustible bodies which exhibit no visible light. Indeed, that light and heat are two distinct existing principles is now finally and demonstrably ascertained and proved by Dr. Herschel's curious experiments of the fact, showing that heat as well as light are each liable to different and peculiar refractions, by which they are separated from each other, and take a separate and distinct existence in different space.

Quod restat desideratum est.

XXIII. *History of Astronomy for the Year* 1801. *By* Jerome Lalande.

THE commencement of the 19th century was distinguished by an astronomical event, the discovery of a planet at Palermo in Sicily, by M. Piazzi, on the 1st of January *. It was as small as a star of the 8th magnitude: he observed it during forty days. The observations he sent me arrived too late for us to be able to follow it, and we were obliged to calculate its orbit from his own observations alone. Burckhardt, Olbers, Bode, Piazzi, and Gauss, have found that to represent these observations it is necessary to suppose that

* I here employ the calendar of all nations, being persuaded that the French government will soon renounce the new calendar, which is not understood, and cannot be adopted either by our neighbours or by the majority of the French themselves.—L.

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it revolves in four years. The following are the elements found by Burckhardt and Gauſs:

Inclination	- - 10° 47′	Inclination	- 10° 36′ 57″
Node	- - 2ˢ 20° 58	Node	- - 2ˢ 21 0 44
Aphelion	- 2 9 0	Epoch	- - - 2ˢ 16° 28′
Paſſage of the aphelion Jan. 1, 1801	8 hours	Mean anom.	- 3 15 55
Eccentricity	- 0·0364	Aphelion	10ˢ 26° 27′ 38″
Semi-axis	- - 2·574	Eccentricity	- 0·0825017
Revolution	- 4·13 years	Equation	- - - 9° 28′
		Diſtance	- - - 2·7355

The difference of theſe elements appeared to me to throw ſome doubt on the reality of this orbit of four years; but in the beginning of the year 1802 it was perfectly confirmed, and we now have a ninth planet.

On the 25th of October we received a printed memoir of Piazzi, with his obſervations and calculations. As he hopes that this ſtar will be acknowledged to be a planet, he has given it the name of Ceres Ferdinandea, in honour of the king of Naples; and Bode wiſhes it to be called Juno: as for my part, I ſhall call it Piazzi, as I gave the name of Herſchel to the planet diſcovered in 1781. The pagan deities are no longer intereſting, and adulation pleaſes only the perſon who is the object of it.

On the 12th of July, in the evening, Meſſier, Mechain and Bouvard, each found a ſmall comet near the head of the Great Bear; and it appears that it was ſeen the preceding night by Pons, concierge of the obſervatory at Marſeilles. The Board of Longitude has given him the 600 francs which I depoſited in the hands of a notary for the perſon who ſhould diſcover a comet; the three able aſtronomers of Paris having themſelves judged that the artiſt ought to be encouraged.

John Louis Pons was born at Peyre, a village in the department of the Upper Alps, on the 24th of December 1761, and has reſided at the obſervatory of Marſeilles ſince the 3d of February 1789: his conduct, addreſs, and intelligence, have procured him much eſteem from the director of the obſervatory. He conſtructed the night-teleſcope, with which he diſcovered the comet of the 11th of July 1801, on the model of a teleſcope by George Adams, which is at the School of Navigation, Marſeilles. The Board of Longitude have ſent him a better one.

I have had a new proof of the utility of the fifty thouſand ſtars which I have procured to aſtronomy, and of the exact poſitions which my nephew Lefrançois has fixed for the ſtars formerly obſerved. Several of them have ſerved to determine the places of the new comet, which Meſſier, Mechain and

Bouvard followed with assiduity, and the orbit of which will be very well determined, though it appeared only ten days.

Thulis has sent me seven observations, from the 12th to the 21st of July, deduced only from the azimuths and altitudes, without having been able to compare them with stars; but we were more fortunate at Paris, and Mechain had sufficient data to calculate the elements in the following manner by a first sketch:

Inclination	= 25°	Perihelion	- 6ˢ 11°
Node	- 0ˢ 8	Distance	- 0 3
		Passage August 7, 15 hours.	

This small comet, found almost at the same time by four persons, proves that it is not difficult to discover comets. Three or four have been seen in the course of a year; and if a few amateurs would employ themselves in searching for them, it is probable that the number would rapidly increase. This is still a desideratum in astronomy; it is humiliating for us, that we do not know whether it is by thousands or tens of thousands that comets ought to be counted, and whether they return, or lose themselves in the immensity of the universe.

Nothing is necessary but a common telescope to search for and find comets, and to point out their situation to astronomers. I suppose that the observer is provided with a wooden quadrant of two feet radius, which any carpenter can make, and that a meridian has been traced out with a large circle on the floor; that the circle is divided into degrees, and that the instrument is directed towards the place where the comet is. Both the altitude and distance from the meridian will be thus found for the time of observation. Nothing more will be necessary to enable astronomers to find a comet which may have been announced. To find comets, it is not necessary, therefore, to know the stars. But there are a hundred nebulæ which have some resemblance to small comets. Those who wish to distinguish them must have recourse to the Celestial Atlas *, where they are all marked. This study will neither be long nor difficult. The Atlas of Berlin is much more complete: we shall give an account of it hereafter.

The night-telescope employed by Messier, and with which he has already found twenty comets, is two feet in length, and has an aperture of $2\frac{1}{2}$ inches: it has three eye-glasses. The first next the eye has a focus of $2\frac{1}{2}$ inches, and 10 lines of aperture; the second 9, and the third $9\frac{1}{4}$ inches. There are 10 lines between the two, and 5 inches between the pre-

* A Paris, chez Lamarche, rue du Foin.

ceding and the ſecond. There is a diaphragm of 14 lines between the firſt and the ſecond eye-glaſs, at the diſtance of 2 inches from the former, and 3 from the ſecond. This teleſcope magnifies only five times, but it has a field of four degrees. One of the ſame kind may be conſtructed for 70 or 80 francs.

Burckhardt alſo has calculated the orbits of the comets of 1763, 1771, and 1773; and for the ſecond he has found a hyperbolic orbit.

The comet which Meſſier diſcovered on the 14th of June 1770, reſpecting which Burckhardt made long and learned calculations, ſeems to have a ſmall circular orbit of five years ſeven months. However, this comet was not ſeen before 1770, nor has it appeared ſince. This can be aſcribed only to great changes in its orbit.

Muſt we then, after having aſſerted, in the 18th century, that all the comets return, aſſert in the 19th that comets do not return, that of 1759 excepted?

On this account I no longer think but of comets; I ſpeak of nothing but comets; and the only thing I recommend to my correſpondents is to ſearch for them, as I write to them that the only thing wanting to aſtronomy is the knowledge of theſe bodies.

On the 15th of May I had the pleaſure of receiving the firſt copy of my *Hiſtoire Celeſte Françoiſe*, the fruit of twelve years labour, which terminates the 50,000 ſtars, on which my nephew Michel Lefrançois has employed the beſt part of his youth. It contains alſo obſervations made by D'Agelet before he ſet out on his voyage round the world; and thoſe by which Darquier, aged 83 years, terminated his glorious aſtronomical career.

The obſervations of Tycho, Flamſteed, Picard, Lacaille, and Maſkelyne, have been the foundation of all the progreſs made in aſtronomy. The moſt profound theories and the moſt learned calculations cannot do without them, and cannot diſpute with them in regard to importance or duration. Obſervations alone will ſurvive us; and obſervers, whom mankind too often affect to undervalue, may conſole themſelves, that they will be the only aſtronomers to whom, long after their death, the praiſes and gratitude of our ſucceſſors and of poſterity will be addreſſed.

Lefrançois Lalande, my nephew, continues his obſervations, together with the calculation of 3000 declinations and 1000 right aſcenſions of the principal ſtars, each obſerved ſeveral times. Theſe long and painful labours have obtained for this able obſerver a place in the National Inſtitute. On

the 26th of December madame Lefrançois Lalande continued the reduction of the 50,000 ſtars; an immenſe labour, to which ſhe has devoted herſelf with courage, and which her pregnancy even has not interrupted. Their ſon is preparing to tread in their ſteps, and already calculates with ſome ſucceſs. I hope that Iſaac Lalande will be the third aſtronomer of his name.

Delambre has obſerved ſeveral declinations with the multiplying circle. Piazzi has announced to us a catalogue of 700 ſtars, which he obſerved at Palermo; and Cagnoli is preparing a catalogue of 500 ſtars, which he obſerved at Paris and Verona with particular care.

Vidal, whoſe courage and exactneſs I have ſo often celebrated, has ſent me the continuation of the auſtral ſtars, which are not well ſeen at Paris; the circum-polar ſtars which were wanting, and a very ſingular *triduum*. On the 23d of April and the following days he obſerved all the planets every day. He has joined to them obſervations of Mercury and the ſun at the two ſolſtices with an ingenious compaſs, which he employed to make a great number of obſervations on the declination of the magnetic needle.

Bourg, an aſtronomer of Vienna, who gained the prize propoſed by the Inſtitute on the inequalities of the moon, continues to employ himſelf on that ſubject. He has recalculated, with 3000 obſervations, the 24 inequalities of the moon; and has added new ones, which were pointed out to him by Laplace according to his theory. Theſe tables arrived on the 8th of December; the errors do not amount to 15″; and the prize of 6000 francs, propoſed by the Board of Longitude to the firſt who ſhould make good lunar tables, will be well merited by this able and courageous aſtronomer. The Board of Longitude is ſtill employed in verifying them; but all the obſervations lately made at Gotha confirm the exactneſs of theſe tables. For it was at the obſervatory of Gotha, the ſanctuary of aſtronomy in Germany, that M. Bourg finiſhed his labour. Baron von Zach had invited him thither, that he might enjoy all the comforts and conveniences he could deſire.

What remains to be done in regard to the theory of the moon depends, perhaps, on ſome terms in which the higher powers of the eccentricities and forces muſt be employed. Burckhardt is now engaged in reſearches on that ſubject.

Arabian obſervations of the 10th century had been employed for the motions of the moon. The manuſcript which I fortunately found among the papers of Joſeph Deliſle made us deſirous of obtaining he original, which was at Leyden; and

and the Batavian minister sent it to us. C. Cauſſin examined this manuscript: but it is not complete; it contains only observations already known. We found in it none of the information so much wished for respecting the instruments of the Arabians, and their method of observing; but it has furnished us with some interesting corrections for our copy, which is now printing in Arabic and French at the printing-office of the Republic by order of the minister of the interior. The observations of the summer solstice have again assured to us that the obliquity of the ecliptic is 23° 28′ $6\frac{1}{2}''$, being $5''$ more than in my tables. The multiplying circles give us the truth within a second; and I think we can with certainty say that the diminution, which has occasioned so much dispute, is $33''$ per century; very far from that which C. Cassini assigned from bad observations made at the observatory with bad instruments.

The winter solstice gives us $8''$ less; but there is reason to believe that this arises from the refraction, which is not yet well enough known for small altitudes. Though the obliquity is nearly decided, the Academy of Berlin has still proposed this variation as the subject of a prize for the year 1802. It requires the most interesting researches and explanations in regard to this subject, where several points still remain to be cleared up.

All the planets have been eclipsed by the moon in the course of this year, as has been observed by Reggio in the Ephemerides of Milan. This phænomenon is rare. We were not able to make a good observation but of the eclipse of Venus on the 13th of May. We were, however, indemnified by the eclipses of that beautiful star the Virgin's Spike, observed in many places on the 30th of March and the 24th of May; which enabled me to verify the longitudes of several countries. Eclipses of four stars of the first magnitude are phænomena of very great importance for all determinations of this kind.

I have continued to discharge the task, which I imposed upon myself forty years ago, of calculating all the eclipses of the sun and stars hitherto observed, and from which astronomers had neglected to draw conclusions on account of the length of the calculations. I have corrected the longitudes of Rome and Middlebourg, and of the new city of Washington in America.

M. Leduc of Sermonetta, Gaetani, and Conti have sent me observations from Rome. M. Ciccolini has sent me some from Florence; and the day on which the king of Etruria came to the Institute, I had the pleasure of presenting

to him a determination of the longitude of Florence, which had been very badly determined, notwithstanding the celebrity of that capital, and the great number of eminent men it has produced.

M. Kautsch, a piarist of Leutomischel in Bohemia, has finished an immense labour on the eclipses of the sun. He has calculated, for the whole of the 19th century, charts in which are seen all the circumstances of these eclipses for every country of the earth where they are visible, in the same manner as they have been inserted in our Ephemerides since 1750, and at present in the *Connoissance des Temps* by the care of C. Duvaucel. I wished to have the means of publishing the labour of M. Kautsch, whose zeal and ability deserve every praise.

C. Goudin has also published an analytical method for eclipses: he has applied it to the eclipse of 1847, which will be the most considerable of this century, and has calculated all its circumstances for the whole surface of the earth.

The conjunctions of the planets are not interesting to astronomers, but they afford a spectacle to the public, especially when connected with other events. Messier, therefore, remarked, that when the cannon announced to us the happiness of peace on the 3d of October, the Moon, Venus, Jupiter, and Saturn, were near that beautiful star in the Lion's heart.

We no longer live in times when such phænomena are considered as of importance, but in 1186 the astronomers announced terrible revolutions in consequence of the conjunction of all the planets. I engaged C. Flauguergues to calculate this phænomenon exactly by our new tables, and he has found that on the 15th of September at 5 h. 2 m. all the planets were between 6 signs, and 6 signs 10 degrees of longitude.

These are not complete conjunctions; exact conjunctions of all the planets are incalculable; a sketch of these returns, in which I employed only days for the duration of the revolutions, gave me 17 millions of millions of years as the interval between one conjunction and another. What would the case have been, had I taken into account the hours and minutes?

As the tables of Mars were the most incorrect, C. Lefrançois has been employed on them six months; he has calculated all the observations, and has employed all the new perturbations. He has carried his precision to tenths of seconds, and has at length constructed tables of Mars, which will leave very little to be wished for, and which have

appeared

appeared in the *Connoissance des Temps* for the year 12. I have seen, with pleasure, my immediate successor and dearest pupil pursue the labour which my master Lemonnier made me undertake, fifty years ago, in imitation of Tycho Brahe, who began his researches on the planet Mars, and who put Kepler in the way of making his discoveries by means of the same planet. He will soon employ himself on tables of Venus, taking into account her perturbations.

For Saturn, the error has been found + 1″ in longitude and — 9″ in latitude. Delambre has made new researches to correct the error of 30″ in the tables of Jupiter, but it has been found in the observations made for 60 years: we must therefore search for the cause of it in the theory, and in some new irregularities.

Bouvard has terminated his calculations of all the perturbations of the planets, each by the action of all the rest, according to the theory of Laplace. The result will be new tables, which will be still more correct.

C. Burckhardt has made an analytical and numerical calculation of the terms of the fifth order, which were not before taken into account in consequence of the length of the calculations. He has found that these terms increase the great irregularity of Saturn by one minute.

C. Chabrol has calculated observations of the sun, and has found that 7″ must be deducted from the longitudes given by our tables. But Delambre has undertaken to calculate 7 or 800 observations of Bradley, applying 8 or 10 new equations furnished by calculations of the attraction. The eccentricity of Jupiter and the earth give equations for the sun which amount to from 8 to 9″. We shall therefore soon have new tables of the sun still more correct than those published by Delambre ten years ago, and to which it appeared that nothing could be added.

For mercury, the error in my tables did not exceed 10″. An observation of Venus on the 24th of May gave me an error in the tables of + 30″. This seems to indicate that we ought to deduct 12″ from the epoch, but that the equation of the orbit is good.

The *Connoissance des Temps* for the year 12 (1804), which has just appeared, contains every thing most interesting that has occurred in regard to astronomy during the last year. Curious researches respecting the theory of the moon, by Laplace; new tables of Mars, by Lefrançois-Lalande; a new catalogue of stars reduced, making the number 11,300, being a continuation of those in the preceding volumes; important observations, tables, and calculations, by Me-

chain, Delambre, Chabrol, Vidal, Thulis, Flauguergues, Ciccolini, Duc Lachapelle, Burckhardt, Bernier, Humboldt, Quenot, and several calculations by myself; with a notice of the most important works that have appeared in the course of a year.

The Ephemerides of Vienna for 1802 contain a fourth series of longitudes determined by Triesnecker, who has calculated all the eclipses of the sun and stars which have been observed; a considerable and important labour, which remained to be undertaken. M. Triesnecker has given us at the same time a table of all the preceding results in regard to the position of towns where eclipses have been observed.

The third volume of the Memoirs of the Institute, the eighth volume of the Memoirs of the Italian Society, the Ephemerides of Berlin for 1803 and 1804, those of Milan for 1801, and the Journal of Baron von Zach during the whole year, have continued to furnish interesting observations and new memoirs. Baron von Zach has procured the observations made by Liesganig at Vienna from the year 1755 to 1774; and those which Niebuhr made in the Levant in 1761, and which he did not publish. We have received the Memoirs of the Academy of Berlin for 1796 and 1797, and the sixth volume of the Memoirs of Turin; but they contain nothing on astronomy.

The observatory of Paris, having acquired new instruments, has been put into activity by Mechain and Bouvard; and the Board of Longitude intends to print the observations of 1801 in the same form as those of Greenwich.

Caroché having finished the telescope of 22 feet, Tremel is employed in constructing a stand to support it; and the platform on which it is to be placed is already in a state of great forwardness. On all occasions we have enjoyed the happiness of having for minister a man long celebrated by his attachment to the sciences, and who, in order to be useful to them, wanted only that influence of which he is so worthy.

The transit telescope which Joseph Delisle had, in 1748, erected at the hotel de Clugny, and with which I as well as Messier made my first observations, had become almost useless by rust. The Board of Longitude wished that it should be reconstructed with platina, and our colleague Messier will have new assistance for his useful observations.

Lenoir has shown at Paris, during the public exhibition of the year 9, that the French industry is not inferior to the English; he received from government one of the twelve gold medals distributed to the most meritorious of our artists.

The

The Board of Longitude has ſent a quadrant to Flauguergues at Viviers, and one to Dangos at Tarbe, to enable them to make more correct and more continued obſervations.

Flauguergues has already employed his obſervations to determine the latitude of his obſervatory 44° 29′ 22″, greater by 18″ than what was given by the triangles; he has aſſiduouſly continued to obſerve the eclipſes of Jupiter's ſatellites; he has obſerved the ſpots of the ſun, which were frequent this year, and has calculated a great many poſitions of the ſtars.

Chabrol has communicated to us a new analytical method for eclipſes, and has calculated ſeveral; he has alſo verified the tables of Mars and Mercury by obſervations in the preſent year. He has reduced 600 obſervations of the ſtars, and has calculated 600 longitudes in the fundamental catalogue: in a word, he ſhows himſelf a young zealous co-operater, curious and void of ambition, who is entitled to our grateful acknowledgments.

C. Mougin, curé of La Grande-Combe-des-Bois, in the mountains of the department of Doubs, has ſent us a large table of preceſſions; that is to ſay, of the annual changes of the ſtars in right aſcenſion, according to the data with which I furniſhed him. For thirty years paſt we have received from this worthy paſtor marks of zeal, application, curioſity, and courage, very rare, eſpecially in the deſerts.

Dr. Maſkelyne has ſent us his obſervations of 1800, a continuation of the valuable collection he has been making for 36 years; and he has announced to us the Nautical Almanac of 1806.

[To be concluded in our next.]

XXIV. *Some Account of the Poiſonous and Injurious Honey of North America.* By Benjamin Smith Barton, *M. D.**

In the year 1785 I had an opportunity of obſerving ſome of the diſagreeable effects of our wild honey upon ſeveral perſons who had eaten of it, in the weſtern parts of Pennſylvania, near the river Ohio. From theſe effects I was perſuaded, that a ſubſtance which is generally conſidered as entirely innocent, is capable of doing much injury to the conſtitution. I was, therefore, induced to pay ſome

* Read before the American Philoſophical Society, and communicated to the Editor of the Philoſophical Magazine by the author. It will form part of the fifth volume of the American Tranſactions.

attention

attention to the ſubject. The reſult of my inquiries I now communicate to the Philoſophical Society.

It is not neceſſary to make any remarks on the fabric of honey. It may be ſufficient to obſerve, that the honey will always partake, in a greater or a leſſer degree, of the ſmell, the taſte, and general properties, of the flowers from which it is obtained. This obvious fact ſhould have ſolicited more of the attention of thoſe whoſe employment it is to raiſe large numbers of bees for the purpoſe of obtaining the valuable product of theſe little inſects. But, in this country at leaſt, hardly any attention has been paid to the ſubject. Perhaps, the following looſe hints, by pointing out ſome of the ſources from which an ill-flavoured or pernicious honey is obtained, may be of ſome ſervice to the new or remote ſettlers of our country.

I muſt obſerve, that in theſe hints I do not mean to include among the diſagreeable conſequences of the eating of honey, the occaſional effect of its purging: for although, as I ſhall preſently obſerve, a purging is one of the common effects of the poiſonous honey, yet the moſt innocent honey will often induce the ſame ſtate of the body, when it is eaten in large quantities, or when it meets with an irritable ſtate of the bowels.

The honey which I call deleterious or poiſonous honey, produces, as far as I have learned, the following ſymptoms, or effects: viz. in the beginning, a dimneſs of ſight or vertigo, ſucceeded by a delirium *, which is ſometimes mild and pleaſant, and ſometimes ferocious; ebriety, pain in the ſtomach and inteſtines, convulſions, profuſe perſpiration, foaming at the mouth, vomiting, and purging; and, in a few inſtances, death. In ſome perſons, a vomiting is the firſt effect of the poiſon. When this is the caſe, it is probable that the perſons ſuffer much leſs from the honey than when no vomiting is induced. Sometimes the honey has been obſerved to produce a temporary palſy of the limbs; an effect which I have remarked in animals that have eaten of one of thoſe very vegetables † from whoſe flowers the bees obtain a pernicious honey.

Death is very ſeldom the conſequence of the eating of

* An intelligent friend of mine related to me the caſe of a perſon who, for a ſhort time, was ſeverely affected from the eating of wild honey, in Virginia. He imagined that a perſon ſeized him rudely by one arm, and then by the other. After this he fell into convulſions, from which, however, he recovered in about an hour. It was imagined that this honey was obtained from a kind of poiſonous muſhroom.

† The Kalmia latifolia.

this

this kind of honey *. The violent impreſſion which it makes upon the ſtomach and inteſtines often induces an early vomiting or purging, which are both favourable to the ſpeedy recovery of the ſufferer. The fever which it excites is frequently relieved, in a ſhort time, by the profuſe perſpiration, and perhaps by the foaming at the mouth. I may add, that as the human conſtitution reſiſts, to an aſtoniſhing degree, the effects of the narcotic and other poiſonous vegetables that are beſt known to us, ſo we need not wonder that it alſo reſiſts the effects of the deleterious honey which is procured from ſuch vegetables.

It deſerves to be mentioned, that the honey which is formed by two different hives of bees in the ſame tree, or at a little diſtance from each other, often poſſeſſes the moſt oppoſite properties. Nay, the honey from the ſame individual comb is ſometimes not leſs different in taſte, in colour, and in its effects. Thus, one ſtratum or portion of it may be eaten without the leaſt inconvenience, whilſt that which is immediately adjacent to it ſhall occaſion the ſeveral effects which I have juſt enumerated.

I have taken ſome pains to learn what are the ſigns by which the deleterious honey may, at firſt view, be diſtinguiſhed from innocent honey. I am informed that there is no difficulty in the matter.

The poiſonous honey is ſaid by ſome to be of a crimſon colour; by others, it is ſaid to be of a reddiſh-brown colour, and of a thicker conſiſtence than common innocent honey.

Theſe are the ſigns by which, I am told, the moſt experienced hunters in the ſouthern parts of North America are enabled to diſtinguiſh pernicious from innocent honey.

On a ſubject ſuch as this, I feel every diſpoſition to pay a good deal of deference to the experience of an American hunter. Even philoſophers may obtain much uſeful information from hunters, however wandering their life, however rude their manners. It is in the power of our hunters to enrich natural hiſtory with many important facts. But we ought not, I preſume, to confide implicitly in every thing they tell us.

I have good reaſons for doubting whether the ſigns which I have mentioned will enable us, in every inſtance, to determine whether honey be poiſonous or innocent.

The honey of the bee, undoubtedly, ſometimes partakes of the colour of the flowers from which it is gathered. The bees gather honey from many flowers of a crimſon colour,

* We ſhall afterwards ſee that not one of Xenophon's men died from the deleterious honey which they had eaten, in large quantities, on the ſhores of the Euxine ſea.

and

and from many flowers whoſe colour is a reddiſh brown. In theſe caſes, it is probable that the honey will ſometimes borrow, in ſome degree, the colour of the flowers. Yet there are many crimſon-coloured and reddiſh-brown coloured flowers that are perfectly innocent. The honey obtained from them will, I preſume, be innocent alſo. Mr. Bruce ſays he was ſurpriſed to ſee, at Dixan, in Abyſſinia, "the honey red like blood; and nothing," he remarks, "can have an appearance more diſguſting than this, when mixed with melted butter *." Nothing is ſaid, by this author, that can lead us to ſuppoſe that the Dixan honey was poiſonous. From the manner in which it is mentioned, it is pretty evident that it was not poiſonous. Linnæus informs us, that in Sweden the honey, in the autumn, is principally gathered from the flowers of the erica, or heath, and that this honey is of a ſomewhat reddiſh colour; and accordingly, he obſerves, thoſe provinces of the country that are deſtitute of the heath, ſuch as the province of Oelandia, furniſh a white honey †. The great naturaliſt ſays nothing concerning the properties of the heath honey. However, we may preſume, when we recollect the minute accuracy of Linnæus, that this honey did not poſſeſs any dangerous properties, otherwiſe he would have noticed the circumſtance. Whilſt I reſided in Edinburgh I had the honey from the Highlands frequently brought to my table. I often remarked that this honey had a dirty browniſh colour; and I was told that it was chiefly procured from the different ſpecies of erica, perhaps principally from the "blooming hather ‡," which abound in the Highlands. I never heard the people in Edinburgh, although they conſume large quantities of this honey, complain that it poſſeſſes any noxious property. If it were actively poiſonous or injurious, the quality would have been long ſince obſerved. I well remember, however, that, for two years that I uſed it, it almoſt always rendered me drowſy. Sometimes, indeed, it compoſed me to ſleep as effectually as a moderate doſe of laudanum would have done. A foreigner, who had not been accuſtomed to eat anodyne honey, was better capable of remarking the effect which I have mentioned than the natives, who had been in the habit of uſing it from their infancy. I do not find that this ſingular property of the Scots honey has been noticed by any writer §. I have therefore related it, though

* Travels to diſcover the Source of the Nile, vol. v. or Appendix, p. 151. quarto edition.

† Fauna Suecica.

‡ Burns.

§ Dr. Withering ſays, bees extract a great deal of honey from the flowers

though it rather oppoſes any objection to the ſigns employed by our hunters to diſtinguiſh poiſonous from innocent honey. But he who is ſtudious of truth ſhould relate uſeful facts as they are, without regarding what is their connection with a favourite ſyſtem or opinion.

The learned Joſeph Acoſta ſpeaks of a gray-coloured honey-comb which he ſaw in the province of Charcas in South America. The honey of this comb, he ſays, is "ſharp and black." He ſays nothing further of its properties *.

An ingenious friend of mine †, to whom the public are indebted for a variety of valuable information concerning the natural productions of various parts of North America, informs me, that, in the Carolinas and Floridas, the poiſonous honey is often ſo ſimilar in colour, taſte, and odour, to the common or innocent honey, that the former cannot be diſtinguiſhed from the latter. It is owing, he ſays, to this circumſtance that ſo many accidents daily happen from the uſe of the wild honey. He was informed that it is experience alone which enables the hunters and others to determine whether the honey which they find in the woods be poiſonous or innocent. They have obſerved that the injurious effects manifeſt themſelves in a ſhort time after the honey is taken into the ſtomach. They are accuſtomed, therefore, to eat a ſmall quantity before they venture to ſatisfy their appetite. Should this produce *any* diſagreeable effects, they do not think it prudent to continue the uſe of it. But if, in a ſhort time, it ſhould occaſion no inconvenience, they think they may, with perfect ſafety, indulge their appetite to the full.

I have been informed that the poiſonous honey, by boiling and by ſtraining, may be rendered as innocent as any honey whatever. It is likewiſe ſaid, that by long keeping it becomes harmleſs.

The honey of which I am treating is poiſonous to dogs as well as to men.

Hitherto I have not been able to obtain any certain information concerning the means to be purſued in the treatment of perſons labouring under the effects of the poiſonous honey. It is ſaid that the Indians, and ſome of the whites, uſe cold bathing with advantage. It is probable that this

flowers of the *erica vulgaris*, or common heath; and he remarks that, "where heath abounds, the honey has a reddiſh caſt." A Botanical Arrangement of Britiſh Plants, &c. vol. i.

* The Naturall and Morall Hiſtorie of the Eaſt and Weſt Indies, &c. p. 303.

† Mr. William Bartram.

practice has been uſeful. As the effects produced by this honey are ſo ſimilar to thoſe produced by ſeveral narcotic vegetables that are well known to us, ſuch as opium, henbane *, thorn-apple †, &c. it is probable that the ſame means of treatment will be found uſeful in both caſes. Of thoſe means it is not neceſſary to make particular mention in this place.

It would be curious to aſcertain whether the bees are ever injured or deſtroyed by the quaffing of the nectar of the flowers from which they prepare the poiſonous honey. It is probable that they are; and, perhaps, ſome of the diſeaſes of theſe little inſects may ariſe from this ſource ‡. It is true, indeed, that there are ſome poiſonous plants the nectar of which the bees will not touch. This is the caſe with the *fritillaria imperialis*, or crown imperial §. I do not remember to have ſeen bees in or immediately about the flowers of the common roſebay or oleander ‖, in the tube of which there is a fluid which deſtroys thouſands of the common houſe flies. But what is called inſtinct is not always ſure. The bees may prepare an honey from plants that are very injurious to them. The excellent Mr. Evelyn, ſpeaking of the elm, ſays, "But I hear an ill report of this tree for bees, that, ſurfeiting of the blooming ſeeds, they are obnoxious to the laſk ¶, at their firſt going abroad in ſpring, which endangers whole flocks if remedies be not timely exhibited; therefore it is ſaid, in great elm countries they do not thrive; but the truth of which I am yet to learn **."

In South Carolina, in Georgia, and in the two Floridas, but more eſpecially in Eaſt Florida, the inſtances of injuries from the eating of wild honey are more numerous than in any other parts of North America that are known to us.

There is a tract of country included between the rivers

* Hyoſcyamus niger.

† Datura ſtramonium.

‡ Dr. James E. Smith aſſerts that the honey or nectar of plants is not poiſonous to bees. Syllabus to a Courſe of Lectures on Botany, p. 23. I have ſome good reaſon to believe that, ſometimes at leaſt, the contrary is the caſe.

§ Linnæus, ſpeaking of this plant, ſays: "Nulla, excepto Meliantho, copioſiori melle ſcatet planta, quam hæc; ſed apes id non colligunt!" Prælectiones in Ordines Naturales Plantarum, edidit Giſeke, p. 287. Hamburgi 1792.

‖ Nerium oleander.

¶ This is one of the moſt mortal diſeaſes of bees. It is beautifully deſcribed, and the remedies for it mentioned, by Virgil, Georgic. lib. iv. l. 251—280.

** Silva, or a Diſcourſe on Foreſt Trees, &c. p. 133 and 134. Doctor Hunter's edition.

St.

St. Illa and St. Mary's, in Eaſt Florida, that is remarkable for abounding in vaſt numbers of bees. Theſe inſects, which were originally introduced into Florida by the Spaniards *, have increaſed into innumerable ſwarms, from the facility with which they procure their food, in perhaps the richeſt flowered country of North America. In this tract of country the alarming effects of the wild honey are often experienced by the ſettlers, by wandering hunters, and by ſavages.

It is highly probable that this poiſonous honey is procured from a conſiderable number of the flowers of the countries which I have mentioned. A complete liſt of theſe flowers would be acceptable; but ſuch a liſt it will be difficult to procure at preſent. Perhaps my hints may induce ſome intelligent native of the country to favour us with his obſervations on the ſubject. Meanwhile, I am happy to have it in my power to mention ſome of the vegetables from whoſe flowers the bees extract a deleterious honey, not only in the country between the St. Illa and St. Mary's, but alſo in ſome other parts of North America.

Theſe vegetables are the *kalmia anguſtifolia* and *latifolia* of Linnæus, the *kalmia hirſuta* of Walter †, the *andromeda mariana*, and ſome other ſpecies of this genus.

I. Every American has heard of the poiſonous properties of the kalmia anguſtifolia and latifolia. The former of theſe plants is known, in the United States, by the names of dwarf-laurel, ivy, lambkill, &c. It has long been known, that its leaves, when eaten by ſheep, prove fatal to them. The following fact will ſhow that the flowers likewiſe are endued with a poiſonous property.

About twenty years ſince, a party of young men, ſolicited by the proſpect of gain, moved, with a few hives of bees, from Pennſylvania into the Jerſeys. They were induced to believe that the ſavannas of this latter country were very favourable to the increaſe of their bees, and, conſequently, to the making of honey. They accordingly placed their hives in the midſt of theſe ſavannas, which were finely painted with the flowers of the kalmia anguſtifolia. The bees increaſed prodigiouſly, and it was evident that the principal part of the honey which they made was obtained from the flowers of the plant which I have juſt mentioned. I cannot learn that there was any thing uncommon in the appearance of the honey: but all the adventurers who ate of it became intoxicated to a great degree. From this experiment, they were ſenſible

* See Tranſactions of the American Philoſophical Society, vol. iii. no. 31.

† Flora Caroliniana, p. 138.

that

that it would not be prudent to ſell their honey; but, unwilling to loſe all their labour, they made the honey into the drink well known by the name of metheglin, ſuppoſing that the intoxicating quality which had reſided in the honey would be loſt in the metheglin. In this reſpect, however, they were miſtaken. The drink alſo intoxicated them; after which they removed their hives.

In North-Carolina, this ſpecies of kalmia and the andromeda mariana are ſuppoſed to be the principal vegetables from which the bees prepare the poiſonous honey, that is common in that part of the United States.

II. The kalmia latifolia, known in the United States by the names of laurel, great-laurel, wintergreen, ſpoon-haunch, ſpoon-wood, &c. is alſo a poiſon. Its leaves, indeed, are eaten with impunity by the deer*, and by the round-horned elk†. But they are poiſonous to ſheep, to horned-cattle and to horſes. In the former of theſe animals, they produce convulſions, foaming at the mouth, and death. Many of General Bradock's horſes were deſtroyed by eating the leaves and the twigs of this ſhrub, in the month of June 1755, a few days before this unfortunate general's defeat and death. In the ſevere winter of the years 1790 and 1791, there appeared to be ſuch unequivocal reaſons for believing that ſeveral perſons, in Philadelphia, had died in conſequence of their eating our pheaſant‡, in whoſe crops the leaves and buds of the kalmia latifolia were found, that the mayor of the city thought it prudent, and his duty, to warn the people againſt the uſe of this bird, by a public proclamation. I know that by many perſons, eſpecially by ſome lovers of pheaſant-fleſh, the circumſtance juſt mentioned was ſuppoſed to be deſtitute of foundation. But the foundation was a ſolid one. This might be ſhown by ſeveral well-authenticated facts. It is ſufficient for my preſent purpoſe to obſerve, that the collection of a deleterious honey from the flowers of this ſpecies of kalmia gives ſome countenance to the opinion, that the fleſh of pheaſants that had eaten of the leaves and buds of this plant may have been impregnated with a pernicious quality§.

* Cervus Virginianus of Gmelin.

† Cervus Wapiti, *mihi*.

‡ Tetrao Cupido of Linnæus.

§ It is not a new ſuſpicion, that the fleſh of animals that have eaten of the leaves, &c. of deleterious vegetables is ſometimes endued with a poiſonous property. Georg. H. Welſchius, a very learned German writer, quoted by Dr. Haller, (ſee *Hiſtoria Stirpium Indigenarum Helvetiæ Inchoata*, tom. i. p. 443.) ſays, that the fleſh of a hare which was fed with the leaves of the rhododendron ferrugineum proved mortal to the gueſts. This ſpecies of rhododendron is a native of Switzerland, Siberia, and other parts of the Old World.

I have

I have been informed, that our Indians ſometimes intentionally poiſon themſelves with a decoction of the leaves of this kalmia. The powder of the leaves has been employed (but I ſuſpect with little advantage) in the inflammatory ſtage of certain fevers. From experiments made upon myſelf, I find that this powder is ſternutatory.

To ſome conſtitutions the flowers of the kalmia latifolia, even externally applied, are found to prove injurious.

III. The kalmia hirſuta appears to poſſeſs nearly the ſame properties as the two ſpecies which I have juſt mentioned. This pretty little ſhrub is a native of South-Carolina, Georgia, and Florida.

In Georgia and in Florida, this ſpecies of kalmia is ſuppoſed to be the principal vegetable from which the deleterious honey in thoſe parts of our continent is procured.

IV. The andromeda mariana, or broad-leafed moorwort, is a very common plant in many parts of North America. The leaves are poiſonous to ſheep. The petioli, or foot-ſtalks of the leaves, and the ſeeds within the ſeed-veſſel, are covered with a brown powder, ſimilar to that of the kalmiæ. This powder applied to the noſtrils occaſions violent ſneezing*. From the flowers of this plant the bees extract conſiderable quantities of honey; and it deſerves to be mentioned that this honey, as well as that obtained from ſome other American ſpecies of andromeda, has frequently the very ſmell of the flowers from which it is obtained†.

I have already obſerved, that it is highly probable, that the American poiſonous honey is procured from the flowers of a conſiderable number of the plants of the country. I have mentioned but a few of them. But there are many others which I have ſome reaſons for ſuſpecting are alſo capable of affording an injurious honey. Indeed, every flower that is poiſonous to man, and is capable of affording honey, may

* For ſome information relative to the properties of the andromeda mariana, ſee Collections for an Eſſay towards a Materia Medica of the United States, pages 19, 20, 47. Philadelphia, 1798.

† In juſtice to the fine genus of andromeda, I muſt obſerve, that all the ſpecies do not furniſh a pernicious honey. The andromeda nitida or lucida of Bartram affords an abundance of nectar, or honey. The flowers of this ſpecies are called by the country people of Carolina and Georgia, "honey-flowers," not, however, merely from the circumſtance juſt mentioned, but from the regular poſition of the flowers on the peduncle, which open like the cells of a honey-comb, and from the odour of theſe flowers, which greatly reſembles that of honey. This ſpecies grows abundantly in the ſwamps called bay-galls. The inhabitants of Carolina are univerſally of opinion, that it affords the greateſt quantity of honey, and that of the beſt quality.

produce an honey injurious to man; ſince the properties of this fluid are ſo dependent upon the properties of the plants from which it is procured. There is, therefore, more poetry than philoſophy in the following lines of Mr. Pope:

> " In the nice bee, what ſenſe ſo ſubtly true
> " From pois'nous herbs extracts the healing dew?"
>
> Eſſay on Man, Epiſtle i. lines 211 & 212.

I have been informed that in the ſouthern parts of our continent, there is a plant, called hemlock, from the flowers of which the bees prepare a honey that is poiſonous. The flowers are ſaid to be yellow, and the root a deadly poiſon. I do not know what plant this is. Moſt probably, it is ſome umbelliferous plant, perhaps a cicuta, an angelica, or a ſcandix.

Some ſpecies of agaricus, at leaſt ſome fungous vegetables, that grow in the ſouthern ſtates, are extremely poiſonous. As accidents from the uſe of deleterious honey have happened in the ſame countries in which theſe poiſonous fungi grow, it has been ſuppoſed, and aſſerted, that the poiſonous honey is prepared from a dew that collects upon theſe fungi. Perhaps, this ſuppoſition is not entirely devoid of foundation*.

I ſhall now mention a few vegetables from the flowers of which, I think, it will be found, that the bees collect a poiſonous or injurious honey. Theſe are:

* If the celebrated author of the *Recherches Philoſophiques ſur les Americains* be ſtill living, this account of our poiſonous and injurious honey (ſhould my memoir fall into his hands) would afford him ſome entertainment. I would adviſe him to connect the facts, which I here communicate, with the remarks concerning our inſects contained in the firſt volume of the *Recherches* (ſee p. 169 and 170.) I hope, however, that Mr. De Pauw, who, notwithſtanding his love of ſyſtem and his many errors, is certainly a man of great reading, will recollect, that the Greek and Roman writers (as we ſhall afterwards ſee) have ſaid much concerning the poiſonous honey of various parts of the Old World. And now let me add, that in America there is as good honey as in any other parts of the world; and there is not a ſcarcity of this good honey. The honey which is collected from the flowers of the tulip-tree (liriodendron tulipifera), the buckwheat (polygonum fagopyrum), the red maple (acer rubrum), the clover (trifolium), and many other plants, is excellent. The Abbe Clavigero ſays the bee of Yucatan and Chiapa makes " the fine clear honey of Eſtabentun, of an aromatic flavour, ſuperior to that of all the other kinds of honey with which we are acquainted." *The Hiſtory of Mexico*, vol. i. p. 68.† Perhaps on ſome future occaſion I may communicate to the Philoſophical Society a liſt of thoſe indigenous vegetables which, as furniſhing an innocent and excellent honey, are worthy of preſervation in the neighbourhood of apiaries. The liſt is an extenſive one.

† This fine honey, according to the Mexican hiſtorian, is " made from a fragrant white flower like jeſſamine, which blows in September."

I. The

I. The rhododendron maximum, or Pennſylvania mountain laurel. This belongs to a very active genus of plants. We have already ſeen, that one of the ſpecies, the rhododendron ferrugineum, was, long ago, obſerved to produce the ſame effects which have been aſcribed to the kalmia latifolia. Another ſpecies, the rhododendron cryſanthum, has been found a powerful medicine, and has been uſed, in Ruſſia, with much advantage, in the iſchias, in chronic rheumatiſm, and in other diſeaſes; and we ſhall immediately ſee that from another ſpecies a poiſonous honey has been procured in the neighbourhood of the Euxine Sea. The footſtalks of the leaves, and alſo the ſeeds, of our rhododendron maximum are covered with the ſame brown powder as I obſerved covered the leaf-footſtalks and the ſeeds of ſeveral of the andromedæ, and the kalmiæ. This powder in the rhododendron, as well as in the andromedæ and kalmiæ, excites ſneezing, and it is curious to obſerve that a ſneezing is mentioned by Dioſcorides among the ſymptoms produced by the honey about Heraclea Pontica. That honey, as will be preſently ſhown, is procured from the rhododendron ponticum.

II. The azalea nudiflora. This fine ſhrub is well known in Pennſylvania, and other parts of the United States, by the name of wild honeyſuckle. Of its properties I know nothing certain. It has, however, too much of the family face, and is too frequently found in company with the rhododendron maximum, and the kalmiæ, not to make me ſuſpicious that it partakes alſo of the characters of theſe deleterious vegetables. Moreover, a ſpecies of this genus, the azalea pontica of Linnæus, is ſuppoſed to be the ægolethron of Pliny, who mentions it as the plant from which the poiſonous honey about Heraclea Pontica is prepared. The tube of the flower of our azalea is perforated by the large bee, called bumble-bee.

III. Datura ſtramonium. This plant is known by a variety of names, ſuch as Jameſtown-weed, gymſin, ſtinkweed, French-cheſnut. Its active and poiſonous properties are now pretty generally known. Children have often been injured by eating the ſeeds. The tube of the flower contains a conſiderable quantity of honey. This honey is bitter, and has much of the poiſonous ſmell. Bees quaff it. But admitting that it is of a poiſonous nature, it does not follow that our *cultivated* bees (if I may be allowed to uſe this expreſſion) will collect ſo much of this honey as to prove injurious to thoſe who eat of it. But, in particular places, where this plant has been permitted to increaſe to a great degree, large quantities of honey may be collected from it:

and I cannot help ſuſpecting that the uſe of this honey may prove injurious*.

Some of the ancient writers of Greece and Rome have related inſtances of the deleterious properties of the honey of certain countries. The botaniſt Dioſcorides, ſpeaking of the rhododendron ponticum, a ſpecies of the ſame genus to which our mountain laurel belongs, has the following words: "About Heraclea Pontica, at certain ſeaſons of the year, the honey occaſions madneſs in thoſe who eat of it; and this is undoubtedly owing to the quality of the flowers from which the honey is diſtilled. This honey occaſions an abundant ſweating; but the patients are eaſed by giving them rue, ſalt-meats, and metheglin, in proportion as they vomit. This honey," continues the Greek botaniſt, "is very acid, and cauſes ſneezing. It takes away redneſs from the face, when pounded with coſtus. Mixed with ſalt or aloes, it diſperſes the black ſpots which remain after bruiſes. If dogs or ſwine ſwallow the excrements of perſons who have eaten of this honey, they fall into the ſame accidents†."

Pliny has alſo taken notice of this poiſonous honey. "In ſome years," ſays the Roman naturaliſt, "the honey is very dangerous about Heraclea Pontica. It is not known to authors from what flowers the bees extract this honey. Here is what we have learned of the matter. In thoſe parts, there is a plant called ægolethron, whoſe flowers in a wet ſpring acquire a very dangerous quality, when they fade. The honey which the bees make of them is more liquid than uſual, more heavy, and redder. Its ſmell cauſes ſneezing. Thoſe who have eaten of it ſweat exceſſively, lie upon the ground, and call for nothing but cool drinks‡." He then makes the very remarks which I have quoted from Dioſcorides, whoſe words, indeed, as Mr. Tournefort obſerves, he ſeems to have merely tranſlated. The following remark, however, appears to belong to Pliny. "Upon the ſame coaſt of the Pontus, there is found another ſort of honey, which is called mænomenon§, becauſe thoſe who eat of it are rendered mad. It is ſuppoſed, the bees collect it from the flowers of the rhododendros, which is common among the foreſts. The people of thoſe parts, although they pay the Romans a part of their tribute in wax, are very cautious how they offer them their honey‡."

* See the late Dr. Samuel Cooper's Inaugural Diſſertation on the Properties and Effects of the Datura Stramonium, p. 33. Philadelphia, 1797.

† Dioſcorides, as quoted by Mr. Tournefort.

‡ C. Plinii Secundi Naturalis Hiſtoriæ Lib. xxi. cap. 13.

§ From the Greek verb *μαινομαι*, inſanio.

‡ Ibid.

The

The Greeks and the Romans have often described the various plants that were known to them, in such dark and obscure terms, that the botanists of modern times are frequently at a loss to determine, not merely the species but also the genus the ancient writers have mentioned. With respect, however, to the plants which I have just mentioned, the difficulty does not seem to be great. Mr. Tournefort has, I think, shown, in a very satisfactory manner, that the ægolethron of Pliny is the chamærhododendros pontica maxima, Mespili folio, flore luteo of his *Institutiones*, a plant since described by Linnæus and by other botanists by the name of azalea pontica. Mr. Tournefort has likewise shown, that the other plant called by Pliny rhododendros is his chamærhododendros pontica maxima, folio lauroceraſi, flore cæruleo purpuraſcente*. This is the rhododendron ponticum of Linnæus. It is considerably allied to the azalea pontica.

Xenophon has recorded the remarkable effects of some poisonous honey, in his celebrated work called *Memorabilia*.

When the army of the ten thousand had arrived near Trebisond, on the coast of the Euxine or Black Sea, an accident befel the troops, which was a cause of great consternation. "As there were a great many bee-hives," says the illustrious general and historian, "the soldiers did not spare the honey. They were taken with a vomiting and purging, attended with a delirium, so that the least affected seemed like men drunk, and others like mad men, or people on the point of death. The earth was strewed with bodies, as after a battle; not a person, however, died, and the disorder ceased the next day, about the same hour that it began. On the third and fourth days, the soldiers rose, but in the condition people are in after taking a strong potion†."

The same fact is recorded by Diodorus Siculus.

Mr. Tournefort thinks there is every probability that this poisonous honey was sucked from the flowers of some species of chamærhododendros, or rhododendron. He observes that all the country about Trebisond is full of the species of this plant, and he remarks that Father Lambert, Theatin missionary, agrees that the honey which the bees extract from a certain shrub in Colchis or Mingrelia, is dangerous, and causes vomiting. Lambert calls this shrub oleandro giallo, or the yellow rose-laurel, which Mr. Tournefort says is, without dispute, his chamærhododendros pontica maxima,

* Institutiones, &c.

† These are nearly the words of Mr. Tournefort's translation. I am sorry that I have not the original work of Xenophon at hand.

Meſpili folio, flore luteo*; the azalea pontica, already mentioned.

There are ſeveral paſſages in the Roman poets, which plainly ſhow, that they were no ſtrangers to the poiſonous properties of certain kinds of honey. It is not neceſſary to mention all theſe paſſages. But the following are worthy of notice.

Virgil cautions us not to ſuffer a yew tree to grow about bee-hives:

> Neu propius tectis taxum ſine.————
>
> Georgic. Lib. iv. l. 47.

In his 9th Eclogue, the ſame philoſophic poet ſpeaks of the yews of Corſica as being particularly injurious to bees.

> Sic tua Cyrnæas fugiant examina taxos. l. 30.

The honey of Corſica was, as Dr. Martyn ſtrongly expreſſes it, "infamous for its evil qualities†."

The raiſing of bees, for the purpoſes of procuring their honey and their wax, may, at ſome future period, become an object of great importance to the United-States. Surely then it would be a matter of conſequence to attend to the cultivation or preſervation of thoſe vegetables which furniſh an innocent and a well-flavoured honey, and a good wax. But even in a more limited view of the ſubject, ſome knowledge of theſe vegetables ſeems to be indiſpenſably neceſſary. And in the new ſettlement, whither the ſettler has carried his bees,

* See Tournefort's Voyage into the Levant, vol. iii. p. 68. Engliſh tranſlation. London, 1741.

† See his tranſlation of the Georgics of Virgil, note to line 47, in book iv. Dr. Martyn's criticiſms and annotations always demand attention. I greatly doubt, however, if the taxus of Virgil be the common yew, or any ſpecies of that genus. Martyn himſelf allows, that "it does not appear from other writers (beſide Virgil), that Corſica abounded in yews." I have been aſſured, that the yew is not an indigenous vegetable in that iſland, and that it is even rare among the foreign vegetables. It may, indeed, be ſaid, perhaps it was common in the time of Virgil. I would obſerve, that the yew is much leſs poiſonous than has been commonly ſuppoſed. I know not that any modern writer has pretended that the bees procure a pernicious honey from its flowers. Theſe facts give riſe to my ſuſpicion, that the taxus of Virgil was not the yew, or taxus of the modern botaniſts. If not the yew, what vegetable was it? Perhaps, the buxus virens, or box. This vegetable abounds in Corſica, where to this day it is known by the name of *taxo*. The gentleman from whom I received this information aſſured me, that the bees of Corſica are very fond of the flowers of the box, and that the honey from this ſource is reputed poiſonous. The box is, unqueſtionably, a poiſonous vegetable. But there is ſtill a difficulty in the caſe. Virgil mentions both taxus and buxus. I think there can be no doubt that his buxus (ſee Georgic. lib. ii. l. 449.) is the buxus of the modern botaniſts.

where

where improvements are still very imperfect, it cannot be deemed a trivial task to have pointed out some of those vegetables from which an injurious honey is obtained.

The ancients, who, in some respects at least, were equal to the moderns, appear to have paid much attention to this subject. Virgil* and Columella have both told us what plants ought to grow about apiaries. It is unnecessary to repeat, in this place, what the two Roman writers have said on the subject. The *Georgics* of the Mantuan poet are in the hands of every man of taste; and the work of Columella† *should* be read, wherever agriculture engages the attention of gentlemen.

The proper management of bees may be considered as a science. It is not sufficient that bees merely make honey and wax. Their honey may be injurious or poisonous, and their wax may be nearly useless. To assist and to direct the labours of these little insects, the knowledge and the hand of man are required. Let, then, this interested being be at least attentive to his own benefits and pleasures. Let him carefully remove from about the habitations of his bees every fetid or poisonous vegetable, however comely its colour or its form. In particular, let him be careful to remove those vegetables which are noxious to himself. In place of these, let him spread the "marjoram and thyme," and other plants, "the love of bees‡," and his labours will be rewarded. He may, then, furnish his table with an honey not inferior to that of Mount Hermettus, or of Athens; nor to that of Sicily, to which Virgil has so handsomely alluded in the seventh Eclogue:

> Nerine Galatea, thymo mihi dulcior Hyblæ,
> Candidior cycnis, hederâ formosior albâ. L. 37, 38.

XXV. *Description of the Arseniates of Copper and of Iron. By the Count de* BOURNON.

[Concluded from p. 12.]

THE modification which we have just seen the primitive crystal assume at one of its solid angles, and only on one side, sometimes takes place also (only on one side) at its two other angles. Then, if the act of crystallization has continued so long, under the same mode of increase, that the new

* See Georgicorum lib. iv. l. 30—32.
† De Re Rustica, libri xii.
‡ Armstrong.

edges, as AB, (fig. 15.) produced by the replacing of the solid angles, unite together, and give birth to a new equilateral triangle, placed in a direction contrary to the primitive one; and if, at the same time, the crystal has such a length that the secondary planes terminate at the opposite base, and are very acute isosceles triangles, the crystal will present the appearance of a kind of truncated hexaedral pyramid, the base and apex of which will be equilateral triangles. (fig. 18. Pl. IV.) The six triangular planes which compose the pyramid of this crystal, are always acute isosceles triangles; but three of them have their acute angle much smaller than the three others. The sides of the base of this kind of pyramid are opposite to the least acute angles; and its truncated apex is opposite to the most acute ones; the triangular planes being placed alternately in an opposite direction. I have seen several instances of this form; but I never saw such intermediate varieties as the secondary plane, represented by the dotted lines in fig. 15. would give, if it existed at the same time in the three angles.

By a longer duration of the act of crystallization, under the same modification, the plane corresponding to the truncated apex of the pyramid (fig. 18.) becomes progressively smaller; the most acute isosceles triangular planes, which answer to the secondary ones, encroach on the least acute, all which are the primitive planes of the crystal, and the pyramid becomes truly triedral at its upper extremity, whilst it remains hexaedral at the base, on account of those parts of the three planes of the primitive crystal which are still preserved. (Fig. 19.)

By a still more considerable duration of the act of crystallization, the pyramid would become completely triedral, and would not be truncated at its apex. I have never met with this modification so complete; but I have seen the variety represented in fig. 19; which, however, as well as fig. 18. is very uncommon.

The triedral prism is subject to a fourth modification, which takes place at the three edges of one of its two bases or terminal surfaces only, and replaces each of those edges by a plane, much more inclined on the side of the prism on which it is placed than on the terminal surface. (Fig. 20.) I have not been able to determine, in these crystals, the angles formed by these new planes, either with the sides of the prism or with the terminal surfaces; but the varieties belonging to this modification demonstrate that these angles are the same as those which the secondary planes of the solid angles make, either with the terminal surfaces, or with the

edges

edges of the prism on which they are inclined. When these new planes have acquired an increase of sufficient extent to make the primitive planes of the prism totally disappear, and to replace them, the crystal is changed to a triedral pyramid with a truncated apex; the base and truncated apex of which are equilateral triangles. (Fig. 21.) When it happens that the crystal has, at the same time, gone through this modification and that which replaces the solid angles of its other extremity, and these two modifications have commenced at the very origin of the formation of the crystal, there is a particular period of its progress, in which the crystal is lengthened into a hexaedral prism, with acute triangular isosceles planes, having for their bases two equilateral triangular planes, perfectly equal. (Fig. 22.) After this period, if the act of crystallization continues, the crystal assumes the appearance of an extremely acute rhomboid, the acute solid angles of which are replaced, more or less completely, by an equilateral triangular plane, (fig. 23.) and finishes at last by taking the form of a perfect rhomboid. (Fig. 24.)

All these varieties, though less common than those of the first modification, are yet frequently to be met with, excepting that of fig. 22. which is extremely rare, and of which I have seen only two or three crystals: in general, however, the crystals of these varieties are very small, and their form cannot be well seen without the assistance of a magnifying glass.

It frequently happens that two of the elongated triedral prisms (fig. 10.) are closely united to each other by one of the sides of the prism; whence results a kind of macle (fig. 25.) the form of which is a rhomboidal tetraedral prism of 60° and 120°; but there is always discernible, on the terminal surfaces of these prisms, a very fine transverse line, AB, on the small diagonal of the rhomboidal plane of these surfaces; this line shows the place of union of the two crystals of which the macle consists.

Sometimes the two component crystals of this kind of macle belong to the triedral prism, which has a secondary plane in the place of one of its edges; it then has the form either of a hexaedral prism that has four of its sides (two and two in opposition) broader than the others (fig. 26.), or of one that has only two opposite sides broader (fig. 27.), or of a regular one, according to the width of the secondary planes: in all these forms, the line AB, indicating the place of union of the two crystals, is perceptible.

It is not very common, as I have already observed, to

meet with specimens of this species in which the crystals are sufficiently detached to let their form be distinctly seen. In general, the crystals are grouped together in great numbers, and seem to penetrate each other, so as to form mamillæ, more or less round; or they form a kind of indented cylinders, which have some resemblance to the trundle of a mill. In that case, the part of the crystals which appears at the surface of these aggregations, commonly belongs to one of the sides of their prism, either the broad or the narrow one. But, when these aggregations form either a kind of cylinders, or of mamillæ in clusters diverging like a fan, there may be seen, at the two edges of the cylinder, or at the summit of the clusters, the whole of the equilateral triangular terminal planes, or trapezia, of one part of the component crystals.

The specific gravity of this species of arseniate of copper is nearly the same as that of the preceding one; I found it to be 4,280. Its hardness, however, is not so great; it is with difficulty that it can be made to scratch calcareous spar.

The crystals of this species, when they have not undergone any change, are transparent, and of a very beautiful blueish-green colour, or deep verdigrise; but their surface easily becomes decomposed, and turns black; the crystals are then totally opaque. It is indeed very seldom, and only in cavities recently exposed, that crystals can be found which retain their transparency and colour. Yet, as the change they undergo commonly takes place only at the surface, rarely penetrating to any great depth, their original colour may easily be restored, merely by slightly scraping the surface with a sharp instrument.

The above is the only change I have had occasion to remark in this species.

Sometimes, but very rarely, this species is found in the form of small hair pencils, with very delicate fibres; and as, in the specimens in which I observed this variety, the little fibrous tuft had preserved its beautiful verdigrise colour, nothing could exceed the beauty of their appearance.

I have likewise observed this species in a mamillary form, with a compact texture; but this variety, like the preceding, is extremely rare.

The matrix of this arseniate of copper is the same as that of all the preceding species; and that species which is most frequently found with it, is the arseniate in obtuse octaedra. It is also frequently accompanied with that kind of ore which is known by the name of *azure copper ore*.

SECTION

SECTION II.

Arſeniates of Iron.

Muttrell mine, which is immediately contiguous to Huel Gorland mine, in the county of Cornwall, has produced ſome ſpecimens of arſeniates of copper exactly ſimilar to thoſe deſcribed in the former part of this paper. But this mine is ſtill more intereſting to mineralogiſts, on account of a combination found therein, of arſenic acid with iron, and alſo a double combination of that acid with both iron and copper.

The firſt-mentioned of theſe arſeniates ſeems analogous to thoſe cryſtals, or cubes, of a fine green colour, of which ſome ſpecimens had already been found in Carrarach and Tincroft mines, and which Klaproth, in his Memoir upon the Mineralogy of Cornwall, conſidered as belonging to the arſeniates of copper; but, according to the analyſis made by Mr. Chenevix, with all the care which his extenſive knowledge and extreme zeal for ſcience would naturally lead him to employ, it appears to be a true arſeniate of iron, containing only a ſmall quantity of copper; and even that quantity ſeems to be merely an accidental mixture. As, in the ſpecimens from the old mines of Tincroft and Carrarach, the greateſt part of the cryſtals adhered to vitreous gray copper ore, it is poſſible that ſome particles of that ore remained attached to the cryſtals; or, as I have frequently found to be the caſe, that ſome ſuch particles had penetrated into the cryſtals, and that Mr. Klaproth had been thereby deceived, by finding in the button left by the blowpipe a much greater proportion of copper than this ore really contains. The natural decompoſition of this arſeniate, which produces an oxide of iron of a fine reddiſh yellow colour, ſtrongly confirms the reſult of Mr. Chenevix's analyſis.

Gmelin, in his Principles of Mineralogy, printed at Göttingen in the year 1790, had already ſuppoſed that theſe cryſtals could not belong to the ſubſtance which, in mineralogical publications, had been called *arſenical copper ore*. He had conſequently ſeparated them, leaving them, however, among the ores of copper, under the name of *würfel ertz*.

The double combination of the arſenic acid with iron and copper, although it had appeared to exiſt in the arſeniate juſt ſpoken of, in the mines of Tincroft and Carrarach, had not excited the attention of mineralogiſts. It is however poſſible that the tranſparency, the brilliancy, and the pale blue colour of its cryſtals, might occaſion them to be miſtaken for cryſtals of a ſtony nature. Beſides, their ſmallneſs might

eaſily

eaſily cauſe them to eſcape the notice of common obſervation, particularly when they are not in pretty large groups.

The matrix of theſe two arſeniates is exactly the ſame as that of the arſeniates of copper; conſiſting, like that, of quartz, mixed with yellow, gray, and vitreous ores of copper, with oxides of iron, and with miſpickel. The mines of Huel Gorland and Muttrell, although not ſituated in the diſtrict of the tin mines, have yet produced ſome ſpecimens of tin, the cryſtals of which are covered with thoſe of the arſeniate here ſpoken of. Two ſpecimens of this kind are in the collection of Sir John St. Aubyn.

Species I. *Simple Arſeniate of Iron.*

This ſpecies cryſtallizes in perfect cubes, (fig. 28.); ſometimes, though rarely, they are a little flattened; their ſides are ſmooth and brilliant.

The only modification I have obſerved in this form is, that four of the eight ſolid angles of the cube are replaced by an equal number of equilateral triangular planes, ſituated in ſuch a manner, that every one of the ſides of the cube becomes an elongated hexagon, having two angles of 90° each, and four of 135°. (Fig. 29.) Cryſtals modified in this way are very ſcarce. I have never ſeen but one ſuch ſpecimen, which is in the collection of Sir John St. Aubyn. The cryſtals of it are pretty large, and very well defined.

The ſpecific gravity of this ſpecies is 3,000. Its hardneſs is juſt ſufficient to ſcratch calcareous ſpar. Its cryſtals, which are tolerably tranſparent, are of a dark green colour, with a browniſh tinge; ſometimes they are rather yellowiſh; and there exiſt ſome ſpecimens of a brown yellow colour like reſin. I have never ſeen this ſpecies in any other ſtate than that of perfect cryſtallization.

Sometimes, indeed, a decompoſition takes place, which cauſes the cryſtals to paſs into the ſtate of a pulverulent oxide, of a fine reddiſh yellow colour. In this caſe, as the bulk of the cryſtals is conſiderably diminiſhed, there is perceived, upon breaking them, a conſiderable number of ſmall cavities in their ſubſtance. Theſe cavities are analogous to thoſe which appear in the cryſtals of the ſpathoſe ores of iron when they have paſſed into a ſimilar ſtate of decompoſition.

Species II. *Cupreous Arſeniate of Iron.*

The cryſtals of this ſpecies are of uncommon brilliancy, and are perfectly tranſparent. Their form is a rhomboidal tetraedral priſm, having two of its edges very obtuſe, and the other two very acute; but, owing to the minute ſize of

theſe

these crystals, I have not yet been able to determine the measure of their angles. The prism is terminated at each of its extremities by a tetraedral pyramid, which is pretty sharp; and its planes, which are scalene triangles, unite by pairs, forming elongated ridges, which join the acute edges of the prism: in the other direction, they unite, also by pairs, so as to form a ridge which is less elongated, and joins the obtuse edges. Very often the obtuse edges of the prism are replaced by planes (of greater or less extent) equally inclined upon the adjacent ones. (Fig. 31.) Sometimes the acute edges are also replaced in the same manner, but always by planes of less extent. (Fig. 32.)

The above are the only varieties I have observed of this arseniate. Its crystals seldom occur singly, being generally grouped together, in a very irregular manner; sometimes, however, they are so united as to assume a mamillated form, having the pyramids of the crystals which compose the mamillæ all placed upon the surface thereof.

The specific gravity of this arseniate is 3,400.

Its hardness is rather greater than that of the simple arseniate of iron: it scratches calcareous spar with greater facility, but does not scratch fluor spar, or heavy spar.

Its colour is that of a very faint sky-blue; sometimes the blue colour is a little deeper. I have seen some crystals which had the same brown resin colour as the preceding species; but they are very rare.

Hitherto I have never met with this species in any other form than that of a perfect crystal.

XXVI. *Analysis of the Arseniates of Copper and of Iron.* *By* Richard Chenevix, *Esq. F.R.S. M.R.I.A.**

section I.

Arseniates of Copper.

THE endless diversity which the hand of Nature has diffused through all her works, even when she makes use of the same primitive materials, must sufficiently convince us, that, whatever accuracy we may attain in the knowledge of the latter, the means which she employs to form her combinations are still secret. The intellectual eye may indulge in the contemplation of hypothetic systems, which itself has created, and which it alone can behold; but how far removed

* From *Transactions of the Royal Society of London for* 1801.

must

muſt they ever be from truths evident to our ſenſes, and ſupported by palpable experiments!

To follow Nature through the minutiæ of her labours, and behold her reproducing the ſame primitive materials in many different ſhapes, has always been deemed a leſs ſplendid achievement of ſcience, than to diſcover one more of thoſe ſimple ſubſtances, by the union of which ſhe forms the complicated effects we daily admire. Yet to me it appears, that in no inſtance is ſhe more truly wonderful than in the unbounded variety which ſhe has ſometimes produced from a ſmall fund of original reſources, and when we can fairly follow a few primitive ſubſtances through a ſeries of combinations infinitely multiplied.

In addition to the two chemiſts who, as is mentioned in the preceding paper by the Count de Bournon, appear to have had ſome knowledge of the exiſtence of a natural arſeniate of copper, I muſt name M. Vauquelin. In a letter to me laſt year, he communicated the diſcovery of ſuch a ſubſtance in France. Of the different varieties which theſe gentlemen, Meſſrs. Klaproth, Prouſt, and Vauquelin, have examined, I ſhall have occaſion to ſpeak in the courſe of theſe experiments: but it was reſerved for the Count de Bournon to ſtate, in the ſaid paper, with his uſual talent and perſpicuity, the ſcientific detail of the external characters, particularly of the cryſtalline forms, by which he had identified their nature. The free acceſs to the extenſive collections of the Right Hon. Charles Greville and of Sir John St. Aubyn, alſo the eaſy communication with the native ſoil of this mineral, were the peculiar advantages, which enabled the Count de Bournon and myſelf to purſue the mineralogical and chemical reſearches which are ſtated in theſe communications to the Society.

When the Count de Bournon had completed what appeared to him to be the mineralogical claſſification of theſe copper ores, he gave me ſome ſpecimens of each kind, numbered indiſcriminately, for the very purpoſe of excluding prejudice; and it was not till my taſk was ended that we compared our obſervations. If I had been admitted into any previous knowledge of the arrangement dictated to him by the principles of cryſtallography, I ſhould have been afraid that I had merely thought true what I wiſhed to be ſo. But I can, moſt conſcientiouſly, indulge in the ſatisfaction which the according reſults of different means to prove the ſame propoſition naturally excite; and which is juſtly due to the truth of the outward marks, however delicate, yet ſtill to be perceived,

perceived, that Nature has left visible to those who will observe her.

I shall now proceed to offer the result of a chemical analysis, undertaken with a view to determine what confidence the crystallographical arrangement, adopted in the preceding paper, might merit; and to show how far sciences so nearly allied may receive new light and confirmation from reciprocal aid.

I shall confine myself to detail only those general processes which, upon frequent trial, have been found preferable. By reducing to powder any of the arseniates of copper here spoken of, and then exposing them to heat in a platina crucible, the water of crystallization was quickly dissipated. But, as too great a degree of heat volatilized some portion of the arsenic* acid, it was found necessary to moderate the heat; and, in order that every particle of water might be finally expelled, to prolong it. When the diminution of weight was ascertained, the residuum was dissolved in acetous, or, still better, in dilute nitric acid, and nitrate of lead was poured in. Arseniate of lead and nitrate of copper were thus formed, by double decomposition; but, when more nitric acid had been used than was strictly necessary to dissolve the arseniate of copper, no precipitate appeared till the liquor had been evaporated. When the evaporation was pushed too far, part of the nitric acid, contained in the soluble nitrate of copper, flew off; and that nearly insoluble cupreous nitrate, first

* There is no doubt that philologists, who do not consider the principles of the new methodical nomenclature, may, at first sight, think the term *arsinic* objectionable; particularly as previous custom and analogy had given another denomination, *arsenical*, which is the natural adjective of the substantive *arsenic*. They may say that the difference of accentuation alone marks the distinction between the substantive and the new adjective. But every chemist will set the weightier considerations of method and order before such objections. In French, the termination in *ic*, for the substantive, and in *ique*, for the adjective, obviates all confusion. One remark I shall beg leave to offer to the consideration of those chemists who have laboured to adapt to the English language a literal translation of the French nomenclature. It is the genius of the former language to throw the accent as far back as possible; so that, in trisyllabical nouns, the first or second syllable is usually accented; while, in the French language, the accent is generally thrown upon the last; thus, we say *sulphúric acid*, but they say *acide sulphurique*. It was very natural therefore, as in the latter case, to make the accented syllable be that which should denote the particular state of the substance of which they speak. Thus, *sulphurique*, *sulphurcúx*; *nitrique*, *nitreúx*; *sulphúte*, *sulphite*; *nitrúte*, *nitrite*. But, without offending the radical orthoepy of our language, we cannot make the same method subservient to that purpose; for, when we wish to mark the distinction in that manner, we are obliged to wrest the word from its proper pronunciation, and to say, *nitric*, *nitrous*, *sulphurcoús*, &c.

mentioned

mentioned by Mr. Prouſt *, was produced. To obviate both inconveniences, alcohol was added, immediately before the liquor was quite evaporated, and long after the precipitate had begun to appear; in a few minutes, all the arſeniate of lead fell to the bottom, while the nitrate of copper was held in ſolution. Theſe new products being ſeparated by filtration, the ſpirituous liquor was diſtilled; and, from the nitrate of copper, the quantity of that metal contained in the ore was obtained by boiling the ſolution with potaſh or ſoda †.

To

* *Annales de Chimie*, vol. xxxii. p. 26.

† By potaſh and ſoda, I mean thoſe alkalies pure, obtained according to the method preſcribed by Berthollet. I know of no other. It is not that I have any predilection for thoſe identical terms; yet, whatever melioration ſubſequent improvement may introduce in particular caſes, if principles are to be adopted, they ſhould, in general, be ſtrictly adhered to. But it muſt be a violation of them to apply a word, appropriated by common conſent to deſign a pure, and as yet a ſimple ſubſtance, to ſuch heterogeneous mixtures as lapis cauſticus, carbonates of potaſh and ſoda, &c. It is indeed much to be deſired, that the epithets *cauſtic*, *pure*, *ſaturate*, &c. ſhould be regarded as tautology, which they really are. There is no potaſh purer than potaſh. When it is not pure, we ſhould ſay, inſtead of "I took ſo much potaſh," "I took ſo much of a mixture of potaſh, and whatever other ſubſtance is mixed with it." Thus, inſtead of calling lapis cauſticus, cauſtic potaſh, or potaſh, as is often done, we ſhould ſay, "I took ſo much of the mixture of potaſh, ſulphate, muriate, carbonate, and ſulphuret of potaſh; ſiliceous and aluminous earths; iron and manganeſe;" for ſuch I find, by analyſis, lapis cauſticus to be. To all this is added, by apothecaries, a little lime. Yet this is the ſubſtance ſometimes called potaſh.

M. Lowitz's manner does not give potaſh pure enough for delicate analyſes of ſtones. I have never ſeen any prepared by his method, in which I could not diſcover iron, ſilica, alumina, and carbonic acid. To the proofs given by Dr. Kennedy, (in his paper intituled "A Chemical Analyſis of three Species of Whinſtone and two of Lava," in the Edinburgh Tranſactions for 1799,) of the efficacy of his method, I propoſe the following objections: That chemiſt ſuperſaturates by nitric acid, and examines by nitrates of barytes and of ſilver. This will be a ſufficient teſt for ſulphuric and muriatic acids; but carbonic acid may have been preſent before ſaturation. He then evaporates, and, if all is rediſſolvable, concludes there is no ſilica or alumina; but, after ſaturation by an acid, ammonia is a more delicate teſt than evaporation for ſmall portions of thoſe earths.

By treating Dantzic potaſh, or, ſtill better, pearlaſh, with lime, and evaporating in a well plated copper veſſel, a white maſs is left. This maſs, diſſolved as far as it can be in alcohol, and the liquor diſtilled to dryneſs in a plated alembic, gives an alkali of a perfect whiteneſs. In this ſtate it is dangerous to touch it; its action on animal matter is ſo ſudden and ſo violent. It attacks all ſtones with the greateſt eaſe and rapidity. Diſſolved in water, it makes not the leaſt cloud in barytes water, or in a ſolution of nitrate or muriate of that earth; and may be uſed as a very delicate and ſenſible reagent, to diſtinguiſh it from ſtrontian. By ſaturating with an acid, and then ſeeking ſilica or alumina, by ammonia, no trace of them can be found, nor indeed of any thing elſe. I do not ſay, however, that the potaſh is perfectly free from every other ſubſtance; I

believe

To the uſe of alcohol, in order to get rid of the exceſs of acid, as mentioned above, there is not the ſame objection that there might be to evaporation, or to an alkali: it can combine with that acid only which is free; and an exceſs of it can, in no way, affect the metallic ſalts.

I have given the preference to lead, above every other method of combining arſenic, to determine its quantity in any other body, having found arſeniate of lime, which has been hitherto recommended, as well as all other earthy arſeniates, to be nearly as ſoluble in water as ſulphate of lime. Lead preſented alſo much facility as to the proportions of its arſeniates; and a few experiments, inſtituted to arrive at them, afforded ſufficient accuracy. But, firſt, it was neceſſary to aſcertain how much acid a given quantity of metallic arſenic could afford; and, finding that it was in vain to aſpire at a greater degree of preciſion than that which Mr. Prouſt had obtained, I have adopted his reſults. By them it appears, that 133 of white oxide and 153 of acid, contain each 100 of real arſenic, the reſt being oxygen. But, 100 of metallic arſenic, acidified by nitric acid, neutralized by an alkali, and precipitated by nitrate of lead, gave 463 of arſeniate of lead; that is, 100 of arſeniate of lead contain 33 and a fraction of arſenic acid; and, on the other hand, my own experiments informed me, that lead, diſſolved in nitric acid, and precipitated by arſeniate of ammonia, gave a proportion of 63; and 4 were expelled by heat from this ſalt. The compoſition of arſeniate of lead, therefore, is,

Arſenic acid - -	33
Oxide of lead - -	63
Water - - -	4
	100

This experiment, repeated ſeveral times, never gave 1 per cent. difference in the reſults. Another method, which may be deemed ſhorter, and perhaps even more accurate, to analyſe arſeniate of copper, is as follows:—After the quantity of water has been eſtimated, the remainder may be treated by either of the fixed alkalis, which will combine with the acid, and leave the brown, the only real oxide of copper, in the ſame ſtate as that in which it exiſted in the ore; the alkaline liquor may be neutralized as above, and the proportions determined in the ſame manner.

believe it contains a little carbon, produced by the decompoſition of the alcohol, and is therefore a ſubcarburet of potaſh; but carbon can be of no conſequence, in the generality of experiments, in humid docimaſia. The ſame method, employed with carbonate of ſoda, is the only one to procure ſoda in a ſtate of equal purity.

No. I. *Third ſpecies of the preceding paper.* One hundred parts, expoſed to a low red heat, loſt nothing of their weight. Diſſolved in dilute nitric acid, decompoſed by nitrate of lead, and precipitated by evaporation, and then by alcohol, they left a white powder, which, well waſhed and dried, weighed 121. But 121 of arſeniate of lead contain 39,7 of arſenic acid. The nitrate of copper, boiled with potaſh, left a precipitate, which weighed 60. Therefore, there are in this variety,

Oxide of copper - -	60
Arſenic acid - - -	39,7
	99,7

No. II. *Fourth ſpecies of the preceding paper.* One hundred parts, expoſed to a low red heat, loſt 16. Treated as above, they yielded a quantity of arſeniate of lead, correſponding to 30 of arſenic acid; and I obtained 54 of oxide of copper. Therefore this variety contains,

Oxide of copper - -	54
Arſenic acid - - -	30
Water - - - -	16
	100

No. III. *Var. 2 of the third ſpecies.* One hundred parts, expoſed to a low red heat, loſt 18 of water. The 82 remaining, boiled with potaſh, left a reſiduum of a blackiſh brown colour, which weighed 51, and which, examined by the different reagents, was found to be oxide of copper, without mixture. The ſupernatant liquor, and the liquor which waſhed the 51 precipitated, being neutralized and evaporated together, left a precipitate, by nitrate of lead, which weighed 88, and, by the proportions of arſeniate of lead, eſtabliſhed above, indicated 29 of arſenic acid. The proportions in this variety are therefore as follows:

Oxide of copper - -	51
Arſenic acid - - -	29
Water - - - -	18
	98

No. IV. *Var. 5. of the third ſpecies.* This gave, by one or other of the two methods, already deſcribed and applied, as follows:

Oxide of copper - -	50
Arſenic acid - - -	29
Water - - - -	21
	100

No. V.

No. V. *Second ſpecies of the preceding paper.* This is the variety which, according to the deſcription I received from M. Vauquelin, he had analyſed. In his letter to me, he gave no particulars of the method he had employed, but merely ſtated his reſult *. By that, it appears to contain,

Oxide of copper - -	59
Arſenic acid - - -	41
	100

Before the reception of his account, I had found,

Oxide of copper - -	58
Arſenic acid - - - -	21
Water - - - -	21
	100

This induced me to repeat the analyſis with the greateſt care and attention; for I thought, that to differ from ſo great a maſter muſt be to differ from truth; but I conſtantly found 21 of water, and 21 of arſenic acid.

This apparent difference muſt, therefore, depend on the ſtate of dryneſs in which he obtained his acid; or perhaps he eſtimated it with the water; and, if ſo, I am happy to find I agree with him ſo near as one per cent. A greater preciſion, as every perſon familiar with analyſis well knows, is not within the power of chemical exactneſs.

No. VI. *Firſt ſpecies of the preceding paper.* One hundred parts, expoſed to a low red heat, loſt much more than any of the other kinds; the deficit amounted to 35. The uſual treatment gave 49 of oxide of copper, and only 14 of arſenic acid. I repeated this analyſis with ſome portions which had not been expoſed to heat, and never found more than 14 of arſenic acid. This arſeniate contains,

Oxide of copper - -	49
Arſenic acid - - -	14
Water - - - -	35
	98

[To be continued.]

* J'ai analyzé, ces jours derniers, une mine de cuivre d'un vert clair criſtallin, en lames hexaëdres, ſe diviſant en lames menus, et legèrement flexibles, comme le mica; et c'eſt pour cela, que les naturaliſtes l'avoient nommé *mica vert.* J'ai trouvé que ce mineral étoit compoſé d'environ 59 d'oxide de cuivre, et de 41 d'acide arſenique; et c'eſt de véritable arſeniate de cuivre. Paris, Auguſt 30, 1798.

XXVII. *On the apparent Elevation of Objects above the Horizon.* *By Professor* De Luc*.

It is well known, that on the borders of lakes, the banks of broad rivers, and the shore of bays, a certain optical illusion often takes place when the spectator is stationed in a high point of view. The opposite bank, under certain circumstances, is seen as it were floating in the air; and this phænomenon is ascribed to refraction: but in my opinion it proceeds from a different cause.

About two years ago I observed the phænomenon in question in one of those extensive turf moors that occur so often among the heights in Bremerlande, through which I was then travelling. The trees which bordered this moor, towards the horizon, appeared to me to be floating in the air. I at first conjectured, according to analogy, that there must be a lake or river at the end of the turf moor; but the postillion who drove the carriage assured me that this was not the case.

In the month of July this year, being in the same district, I saw a similar phænomenon, and under circumstances which induced me to examine it with more attention. I was on the road between Bremervörde and Brederkesa, proceeding in a north-east direction. It was about nine in the morning; the heavens were covered with light clouds, yet the sun appeared through them a little behind me on the right, and over a turf moor which extended to the horizon, and was bordered with a row of trees. When the phænomenon first appeared, I was travelling down an eminence on a road adjacent to the edge of the moor, which entered it a little way, and then proceeded up an opposite hill. The variations which I observed in the phænomenon as I advanced slowly down the hill, induced me to dismount, that I might be able every now and then to turn back. What I observed on this occasion was as follows:

At the moment when I was about to descend, I could see and observe from the highest point of the hill a very large surface of the turf moor, which extended itself with increasing breadth towards the horizon; the distance of which I estimated at a German mile. From this pretty high point of sight, I could perceive that the trees at the extremity of the moor did not stand exactly in a line, but that they formed a sort of wood, which extended backwards on the other side,

* From *Der Gesellschaft Naturforschender Freunde zu Berlin Neue Schriften*, vol. iii. 1801.

and,

and, on the ſide next to me, had ſome projecting curves, but in ſuch a manner that the row ſeemed uninterrupted and continued.

The firſt thing that ſtruck me as I deſcended, was, that at the bottom of this row of trees, and near the extremity which was oppoſite to me, there appeared an aperture where there ſeemed to be no trees, and where I thought I could perceive the heavens under a line of trees which I conſidered as further diſtant, becauſe their colour was not ſo dark, and becauſe they appeared ſmaller than the trees of the firſt row between which I ſaw them. This ſeparated part of the row ſeemed to be completely floating in the air; the horizon in the interval of the trees of the firſt row was bounded by the ſurface of the turf moor, and I imagined that I ſaw the heavens between this extreme ſurface of the moor and the row of the moſt diſtant trees.

It was at this moment I alighted: I then turned back, aſcended the hill a little, and ſaw the trees which attracted my attention gradually aſſume their former appearance. The whole ſeparated part again ſunk down to the earth, recovered their former dark colour, and alſo their former height; ſo that I was ſcarcely able to diſtinguiſh this part from the remainder of the row. Some traces, however, of the former transformation remained. I was therefore induced to examine the whole row throughout with the greateſt attention; and, on doing ſo, I imagined that I obſerved ſome parts which lay a little more in the back ground, and which, as I conjectured from certain ſigns, would appear transformed in the like manner when viewed from a ſomewhat low point of ſight. In regard to the loweſt ground line of the row of trees, it appeared, on account of the diſtance and thickneſs of the atmoſphere, too undefined to perceive bendings in it: I was not able to obſerve the trunks of the trees, which I diſtinguiſhed from the ſurface of the moor merely by its brown colour being rendered leſs pale by the intervening ſtratum of air, by the trees having a higher poſition, and by their being ſeen through a purer ſtratum of the atmoſphere. On again deſcending, I ſaw thoſe parts of the row which I conſidered as the moſt remote, gradually raiſe themſelves above the horizontal plain; ſo that they ſeparated themſelves from the row in which they ſtood, and ſeemed to float in the air behind the row which now had the appearance of being broken. When I reached the valley, where I was however ſomewhat elevated above the level of the moor, theſe phænomena began to appear in the foremoſt row, where the firſt breaking appeared. I again thought I ſaw the heavens below the trees at the ex-

treme edge of the moor. Theſe trees alſo appeared to be floating in the air, but they did not elevate themſelves above the ground ſo much as thoſe I firſt obſerved. As I advanced further in the valley, my horizon did not any more change in a perceptible manner; though my poſition in regard to certain parts of the row of trees was conſiderably altered, which produced ſeveral modifications of the phænomenon. The elevation of the trees above the horizontal ſurface increaſed the further I proceeded, but gradually decreaſed in thoſe parts from which I receded, and where it had firſt begun, and at length the whole diſappeared when I aſcended the oppoſite hill.

This circumſtance of the phænomenon led me to an obſervation, from which I concluded that refraction had no ſhare in it. The maſſes of the diſtant trees which raiſed themſelves from the ground, were ſituated between parts of the foremoſt row, which for a conſiderable time did not change their poſition; and theſe unchanged parts gave me fixed points, by which I was enabled to eſtimate the elevation of the objects; and it was this eſtimation which conducted me to a knowledge of the nature of the phænomenon.

The intervals where, in conſequence of my progreſſive motion forwards, the heavens appeared under the moſt diſtant trees, raiſed themſelves more the lower my ſtation became on the hill, and the further I advanced in the valley: but the ſummits of the floating trees were not elevated; on the contrary, they ſeemed ſomewhat depreſſed in compariſon of the foremoſt trees. The maſſes became ſhortened below, but at the top they remained on the ſame level. I had ſufficient opportunity to confirm this circumſtance, as, by changing my poſition in a vertical and horizontal direction, I ſaw three different maſſes ſo totally diſappear, that, in the interval which they before occupied in the row of trees, nothing more was to be ſeen but the pure heavens. On one of theſe maſſes diſappearing in this manner, when I retired back from my poſition, there firſt appeared towards the heavens ſome brown ſpots, which were the higheſt ſummits of the trees; and the more I receded, the more it ſeemed as if a curtain dropped down which concealed the part of the heavens before ſeen. But with whatever attention I looked towards the ſpot where this curtain fell down, and where the ſummits of the trees at length diſappeared, I could obſerve nothing but the pure heavens. Had a perſon, without ſuſpecting any thing of this optic transformation, ſeen from the valley the row of trees in the horizon evidently broken, and had again ſeen from the hill without obſerving the gradual change

during the time of advancing, the ſame row once more in the horizon entirely whole, without any interval or break, he would certainly not have believed his own eyes.

Between this phænomenon and that where the coaſts appear elevated in the like manner, I could obſerve no difference in regard to the cauſes which produce them; and the ſame identity is applicable to thoſe phænomena which I obſerved in another moor in the ſame neighbourhood about two years before; only that the trees which formed the horizontal boundary ſtood actually in a line, ſo that the whole row ſeemed floating in the heavens at once, and not in portions at a time. All theſe phænomena then are identically the ſame, and it follows from the circumſtances here deſcribed, that they do not depend on refraction. The cauſe alone lies in the rays of light reflected from the vapours, which in conſequence of this reflection appear ſo dazzling to the eye, that they conceal the objects ſituated below, and preſent the ſame appearance as the heavens. The ſun, as already ſaid, was towards that ſide where I ſaw the phænomenon, and his altitude was conſiderable, as I obſerved the above transformation on the 18th of July, between the hours of 9 and 10. While I obſerved from the valley ſeveral of theſe broken parts of the firſt row of trees, the ſun was in a perpendicular direction over them. The elevation of the maſſes above the ground increaſed in general more and more as my ſituation became gradually lower, and according as each maſs of the objects to be changed approached nearer to a perpendicular poſition under the ſun, in conſequence of my advancing in a horizontal direction. As I deſcended, I had between me and the objects a ſtratum of the atmoſphere, which was always denſer, and conſequently more proper for reflecting the rays. When I advanced in the horizontal plain, this reflection took place more in a ſtraight line; and when I came into ſuch a poſition that the vapours threw back the rays of light to the level of the ſummits of the moſt diſtant trees, I ſaw nothing but this luminous vapour, which loſt itſelf in the heavens, and formed with them one continued field. When I had reached the loweſt point of the plain, where I found a favourable point of view, ſome of the foremoſt trees could then raiſe themſelves from the ground, becauſe they were nearer to me; and to compenſate for this greater proximity, the rays of light muſt have penetrated a much thicker ſtratum of the atmoſphere, and muſt have proceeded from a point ſo much nearer the perpendicular poſition under the ſun.

The preſence of the ſun is the principal condition of this

phænomenon: without the action of the sun's beams, the vapours would not be dense enough to conceal the objects: it is by means of the solar rays that they are made to disappear from the eye. These rays, however, can be transmitted to the eye only under a given angle, in the same manner as a certain angle is required in order to produce the rainbow by refraction. The stratum of air which is sufficiently dense to effect this reflection of the sun, rises to a certain height, but it can be perceptible to the eye only when the vapours actually throw back a great number of rays. As soon as the point of sight corresponding to this effect is obtained, the eye sees only the shining vapour, which it cannot distinguish from the heavens. This disappearance, however, takes place in regard to distant as well as to near objects, and rather at the lower than the upper part of the object, because, in the first case, the direction of the line of sight passes through a larger mass of vapour, and in the second through a denser stratum. The less dense part of the vapour, which did not reflect rays of the sun, but which always grew denser the lower my station became, acting like a veil, which interposed itself between my eye and the trees which had not yet disappeared, made me observe their distance, and contributed to cause the elevated trees to appear as if floating behind the row.

In regard to the circumstance that the tops of the distant trees did not raise themselves above those of the foremost, though their lower parts seemed to rise above the ground as I descended, and that these tops on the contrary seemed somewhat depressed; it arose from the decrease in the elevation of my point of station having a greater ratio to the less distant than to the most distant trees; by which means, the former were raised more in the same proportion above the horizon.

When this phænomenon is observed on the banks of large pieces of water, and when the objects are at different distances from the observer, the truth of the principles here advanced may be confirmed by remarking, that the phænomenon is modified by a change of position; but if the observer can move about in a horizontal direction, it will be of great use to observe the changes of the object in a vertical direction, supposing he has opportunity, on any gradual declivity, of ascending and descending. If the vertical measurement of the objects then changes, so that their total height decreases the more the objects seem to rise from the ground when the observer descends the declivity, this is a sufficient proof that the whole image is not elevated, but that

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its loweſt part diſappears from the eye, behind a maſs of vapour ſo condenſed that it cannot be diſtinguiſhed from the ſky above or the water below.

I entertain no doubt, that this is the real cauſe of all the phænomena of this kind, as I have never been able to comprehend how ſuch phænomena could be produced by refraction.

When the objects which are ſeen under water appear elevated in different degrees; it may be readily conceived that the rays of light proceed from the water, which is very denſe, into air, which is much rarer. When we ſee the celeſtial bodies above the horizon, while they are actually below it, we may eaſily conceive that the rays of light, proceeding in an oblique direction, ſometimes paſs through rarer and ſometimes through denſer ſtrata of the atmoſphere. But, in the preſent caſe, there is no difference in the ſpecific denſeneſs or rarity, and the correſponding power of refraction of the medium through which the ray of light is tranſmitted to the eye; and if an actual elevation takes place by refraction, the action muſt be equally great from water or from a lake, and on the objects oppoſite to them: conſequently the whole maſs of theſe objects muſt appear equally elevated, without any interruption being obſerved in them.

I ſhall here add, that I have been very often deceived in theſe moors, ſo that I thought I ſaw at a great diſtance lakes and rivers, where my drivers aſſured me there was nothing of the kind. This was always the caſe in the morning, and when the ſun by ſtanding over the object gave riſe to the illuſion. Had not the horizon on theſe occaſions been bordered with a row of ſmall hills, I ſhould certainly have believed that I ſaw not only a piece of water, but even the heavens. The hill prevented me from ſeeing at one view the heavens and the luminous vapour, and the vapour ſeen alone appeared to me as an extended piece of water illuminated by the reflection of the heavens.

XXVIII. *Deſcription and Natural Hiſtory of the Wild Goat of the Alps.* *By* M. Berthout Van Berchem *jun.**

THE wild goat of the Alps† is ſtill little known to naturaliſts, though mentioned by ſeveral authors. As it lives on the

* From *Mémoires de la Société des Sciences phyſiques de Lauſanne*, vol. ii.

† *Capra ibex*, Erxleben Siſt. Reg. p. 261. Linn. Hiſt. Nat. p. 95. *Le Bouquetin*, Buffon, vol. xii. p. 136.

ſummits

ſummits of the higheſt mountains, it has been obſerved only by thoſe employed in hunting it, who in general are men fond of the marvellous, and therefore their relations are filled with abſurd tales.

Its figure, however, is better known: yet the deſcription of Daubenton* was taken from a very young individual, and the engraving which accompanies it is not correct. We are indebted indeed to profeſſor Pallas† for a very minute and very good deſcription of the wild goat of Siberia, but this animal ſeems to differ from ours in ſome of its characters. Beſides, profeſſor Pallas does not ſpeak of the Siberian wild goat at its different ages, and has not deſcribed the female, which is ſtill unknown. All theſe reaſons have induced me to make reſearches reſpecting this inhabitant of our lofty Alps, and I flatter myſelf that I ſhall render a ſervice to naturaliſts by making it better known.

Having learned that M. de Watteville, governor of l'Aigle in the Pays de Vaud, had in his poſſeſſion a live young goat, I eagerly embraced that opportunity, and undertook a journey to l'Aigle to deſcribe and make a drawing of it. (See Plate V.) M. de Watteville was ſo kind as to give me every accommodation for the purpoſe, and to him I was indebted for ſeveral intereſting details reſpecting the habits and manners of this animal in its ſtate of captivity.

But to know it in its ſtate of nature, as the domeſticated ſtate always occaſions a conſiderable change in the manners of all animals, it was neceſſary to conſult the wild goat hunters. With that view I proceeded to the country where they live, that I might obtain more certain information, and be able if poſſible to ſeparate truth from falſehood; but I ſhould have loſt much time in fruitleſs reſearch, had I not had the good fortune to addreſs myſelf to one of the ableſt hunters, a man who to great veracity added the ſpirit of a good obſerver.

The wild goat of l'Aigle, the firſt time I ſaw it, in the ſpring of 1784, was two years of age. It came from the valley of Oſt, and had been purchaſed when about five or ſix months old: it was ſuckled by a common goat, to which it was ſtrongly attached: its mild phyſiognomy announced the gentleneſs of its character and of its manners: its hind legs were higher than the fore ones, and its four cloven feet indicated its aptitude for climbing the rocks. Its large and extenſive horns, which ſhaded its ſmall head, and its large limbs

* Hiſt. Nat. de Buffon, vol. xii. p. 166.

† *Ibex Alpium Sibiricarum*, Pallas, Sp. Zool. p. 31.

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and mufcles, feemed to ſhow its ſtrength and its means of defence.

On the firſt view, it had ſome reſemblance to the ſheep in regard to the head, but in the whole of its figure it had more relation to the goat. In comparing it with the latter, it appeared larger and thicker; its forehead was narower and more elevated. Theſe laſt characters diſtinguiſhed it from all animals of the ſame kind. Its head was proportionally ſmaller, the forehead was a little arched before, the eye was quick and of a moderate ſize, the pupil contracted in the ſun like that of the ſheep, forming a rectangle, the baſe of which ſtood in the direction of the greateſt length of the eye; the forehead and upper part of the head were thickly covered with hair; it had no veſtige of beard. But on my ſecond viſit ſix months after, that is to ſay, when it was two years and a half old, it had a ſmall one at the diſtance of an inch and a half from the extremity of the muzzle; it was black, and turned backwards.

Its horns were very different from thoſe of the goat; they were large, near each other at the roots, and at a conſiderable diſtance at the points; bent back and outwards in an arch; they were fourteen inches in length, and eight inches in circumference at the baſe; the two longitudinal ridges, between which was the anterior face of each horn in the old goats, were not both very ſenſible; the interior one was the moſt perceptible; it had two or three large and very prominent tubercles. Theſe tubercles in the large horns of the wild goat terminate the tranſverſal ridges of the anterior face; but here the tranſverſal ridges were not yet diſtinctly marked, though they were perceptible. The poſterior face of the horns was round and ſmooth; but the ſecond time I ſaw it they had conſiderably increaſed. The anterior face and longitudinal ridges were well marked. Near the baſe there were four tranſverſal ridges very prominent, and elevated nearly half an inch: on the interior longitudinal ridge there were ſix tubercles, and the others went on decreaſing to more than half the length of the horn.

The animal was then three feet and a half in length: it appeared to me proportionally higher on the legs than the goat; but this was on account of its being not then well formed, for the old wild goat has ſhort legs in proportion to its length: it had a thicker and ſhorter neck than the goat, the rump was lighter and rounder, the body more plump, and the legs thicker; it had no calloſities on the knees, which in the common goats is one of the marks of its domeſticity: its ears were large, almoſt bare in the inſide; but the edges were furniſhed with whitiſh hairs: the animal always directed them backwards.

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It had two kinds of hair: one a white down, very thick, frizzled and woolly; and large hair, very coarſe and thick on the whole body. It was longer under the neck, and on the anterior parts in general; it had not, like the common goat, a ſort of mane of long hair on the back; the hair on the body was of a ſhining gray fawn colour, and that below the chin, darker than that on the body, inclined to brown. At the lower part of the flanks there was a ſtripe of browner hair which began about the thigh, and extended on the ſide as in ſome ſpecies of the antelope; the belly and inſide of the four legs were white or whitiſh. That black ſtripe on the ſpine found in all wild goats was not obſerved on this animal in the ſpring; but in the month of October it reappeared, beginning at the diſtance of about four inches from the root of the horns, and extending to the tail. The black ſtripe at the lower part of the flanks was alſo better marked, and the hair of the body was of a browniſh gray colour; the hair on the parts below the tail was white, as in the greater part of antelopes. The tail itſelf was brown above and white beneath; it was very ſhort, and covered with long hair: the hair of the body was directed towards the rump, and that on the thighs downwards. The hoofs of this animal were very long; the fore legs were ſenſibly thicker than thoſe behind, and the thighs were ſtrong and fleſhy; its feet were of a rounder form than thoſe of the goat, and better determined; each hoof had a ſort of talon very apparent; the lower face of the hoofs which touches the ground was concave, and terminated by a ſalient edge, principally on the outſide, as in the chamois. The horn of the hoofs was pliable and elaſtic, and equally durable as thoſe of the ſheep.

Dimenſions of the Wild Goat at the Age of Two Years.

	feet.	in.
Length of the whole body from the point of the muzzle to the root of the tail -	3	2
Height before - - -	2	6
Height behind - - -	2	9
Length of the horns according to their bending	1	2
Circumference of the horns at the baſe -	0	8
Length of the tail - - -	0	5

Dimenſions of the Wild Goat at the Age of Three Years.

	feet.	in.	lin.
Length of the whole body from the muzzle to the root of the tail - -	3	6	0
Height before - - -	2	8	3
Height behind - - -	2	11	3

Length

	feet.	in.	lin.
Length of the horns according to their bending	1	4	6
Circumference of the horns at the root -	9	0	0
Length of the beard - -	0	1	6

After having deſcribed the wild goat of l'Aigle, I ſhall now give ſome remarks on wild goats in general.

The length of the horns varies. D'Aubenton gives the dimenſions of a pair of horns preſerved in the king's collection, which were 2 feet 9 inches in length, and 9 inches in circumference at the roots; they had 20 rings or ridges: this is the greateſt length known of the horns of this animal. Their diſtance from each other varies alſo. Sometimes they diverge a great deal, at others very little; their colour is reddiſh or blackiſh, but when the animal becomes old they aſſume a dirty white colour.

It is to be obſerved, that on the large horns of the wild goat the largeſt and moſt prominent tranſverſal ridges and tubercles are towards the roots. This may be eaſily conceived, if attention be paid to the increaſe of this kind of horns, which takes place at the bottom; that is to ſay, the increaſe of each year puſhes upwards that of the preceding year, ſo that the young horns at the end of ſome years form the upper part of the large ones: and as theſe horns always acquire more ſize and breadth at the bottom, the tranſverſal ridge or ring of each year becomes always leſs prominent.

The young are brought forth covered with their firſt woolly hair, but in autumn they begin to be clothed with their long and ſtiff hair: the horns appear in the firſt month, and the animal immediately acquires the black ſtripe on the ſpine.

In ſpring, towards the month of May, all the young and old goats change their hair; that on the back they loſe firſt, and that on the thighs laſt: the black ſtripe diſappears in moulting time, and afterwards re-appears, as we have obſerved: they then acquire red hair inclining to yellow, which, as the ſeaſon advances, becomes dark brown mixed with gray; ſo that in the month of September they are almoſt entirely of that colour, or at leaſt the upper parts of the body, but in winter and towards ſpring their hair becomes reddiſh gray. The belly remains white, and the black ſtripe is always ſeen in the flanks: on growing old they become gray.

The hunters can tell the age of the wild goats by the number of tranſverſal ridges on the anterior face of the horns. Every year they ſay one is produced; and it is by theſe alone that the age can be determined, and not by the other knots or wrinkles with which the horns are covered. But this

method in my opinion is not very certain, ſince the wild goat of l'Aigle, when I ſaw it in the month of October 1784, had four tranſverſal ridges, and two large and extenſive knots, which were to form ridges. According to the calculation of the hunters, therefore, this animal ought to have been four or five years of age, and yet it is certain that it was only two years and a half. Hence, it is not by the horns that the age of the wild goats can be determined with certainty, but by means of the teeth, as is done in regard to ſheep.

It appears that the wild goat lives to a very great age, but not ſo long as might be ſuppoſed from the report of the hunters; for, according to them, it grows to the age of fourteen or fifteen; and as it is known that all animals live ſeven times the period of their growth, it would thence follow, that the wild goat might attain to the age of ninety-eight or one hundred and five years. We ſhall ſoon ſee that this calculation is very much exaggerated.

From what I have already ſaid, it is evident that the hunters are miſtaken when they ſay that the wild goat grows to the age of fourteen or fifteen: what has led them into an error is the number of tranſverſal ridges, which is always greater than the number of years, and conſequently has made the animal appear older than it really was.

Another method, therefore, muſt be employed to determine the period of their growth. The wild goat is not capable of producing its ſpecies before the age of two years, or two years and a half: if that of l'Aigle produced at the age of two years, this ought, perhaps, to be aſcribed to the more abundant nouriſhment with which it was ſupplied in its ſtate of ſervitude, which haſtened prematurely its procreative faculties. And, as it is known that quadrupeds cannot engender before the time when they have nearly attained to their full growth, we may preſume, with ſome ſort of foundation, that the wild goat grows to the age of four years, and perhaps more. This will give 28 or 30 years for the duration of its life, which, in my opinion, is much more natural than that aſſigned by the hunters.

The horns increaſe in length almoſt during the whole life of the animal, but in ſize it grows only till the age of 15 or 16 years. The largeſt have 20 or 30 tranſverſal ridges.

The female wild goat, called vulgarly *étagne*, is a third leſs than the male: it is alſo thinner, and leſs fleſhy. Its horns have very little relation to thoſe of the male wild goat; they are like thoſe of the common goat, and are very ſmall: I have ſeen ſome at Chamonix which were only eight inches in length. They have no anterior face; and, like thoſe of the

the common goat, are furniſhed only with one longitudinal ridge. They are ſomewhat thicker than thoſe of that animal, and have a few ſmall knots on the ridge.

The female, in ſhape, habit of body, and the figure of its horns, has a great reſemblance to a common he goat that has been caſtrated. When ſuckling, it has two teats like the common ſhe goat; and the feet are ſmaller in proportion than thoſe of the male; they are ſharper, and not ſo round. The female wild goat has no beard: according to the hunters, the males are not always provided with one; and, when they are, it is not more than two inches in length. This is one of the differences which appear to exiſt between our wild goats and thoſe of Siberia; the males of which have always a very large beard, and the females a ſmall one.

The wild goat has different cries; the moſt common is a ſhort ſhrill whiſtle, very like that of the chamois, but with this difference, that the whiſtle of the latter is longer: ſometimes it makes a noiſe by breathing through its noſtrils; and, when young, it has a ſmall kind of bleating, which it loſes as it grows old.

The wild goat which I ſaw at l'Aigle was exceedingly gentle and tame: its attachment to the common goat, which ſuckled it, was very ſtrong, though it no longer ſucked: often when it was on the top of the houſe, or in other places inacceſſible to the common goat, and when the latter ſeemed uneaſy, and called to it by her bleating, it immediately returned.

This animal has not a lively and active look; its ſtep is ſlow, and it ſeems to poſſeſs great curioſity; all its movements ſeem to be directed by a ſpirit of reflection, and it appears never to decide till after mature deliberation. Has it this in common with mountaineers, who differ in ſo many reſpects from the inhabitants of the plains? Conſidering the agility of this animal among the rocks, it appeared on the firſt view as if oppreſſed with the weight of the atmoſphere of the level country, and aſtoniſhed to find itſelf on ground for which it was not formed. But, when it had to climb, that heavy and embarraſſed air ſeemed entirely to diſappear: it became agile, and all its movements were performed in a graceful manner; it leaped on the tops of the houſes and on walls with the greateſt lightneſs and eaſe. I beheld it with admiration, in one of the interior courts of the caſtle of l'Aigle, at two leaps mount a wall, without any other ſupport than the ſmall projection of ſtones left by the mortar having dropped off; and, at a third leap, jump upon another wall, which formed a right angle with the former. It began by placing itſelf oppoſite to the ſpot which

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it wiſhed to reach, parallel to the wall along which it intended to proceed, and examined it with great attention; then, as if it had renounced its deſign, it began to traverſe, with ſlow ſteps, the court in which it was confined; but, returning from time to time, it took its ſtation oppoſite to the place it had in view; and ſometimes balanced itſelf on its hind legs, as if to try their elaſticity: at laſt, having repeated this preparation for ſome time, it at once formed its reſolution, darted itſelf forwards, leaped along the wall as already mentioned, and ſoon reached the ſpot it intended. It has been ſeen ſtanding on the upper edge of a door. It choſe for its lodging a place under the roof of the higheſt tower in the caſtle.

This animal was not miſchievous; and if it often preſented its horns, it was only that they might be ſcratched, of which it ſeemed very fond. It knew thoſe who ſcratched it, and often preſented its head to them for that purpoſe. When it wiſhed to ſtrike with its horns, it never did ſo directly forwards, but by riſing up on its hind legs and ſtriking ſidewiſe.

As it occaſioned ſome damage by its leaping about, it was found neceſſary to tie its legs. This ſtate of reſtraint appeared to me to have had an influence on its phyſical conſtitution, as it afterwards became meagre. Born to enjoy liberty, this animal cannot endure confinement: I ſhould even have imagined that a part of its docility aroſe from its weakneſs, had I not known that this gentle and ſociable animal can be eaſily tamed. The following trait, which ſhows how ſuſceptible it is of being tamed, deſerves to be known:

A guide of Chamonix, being employed to conduct to Chantilli, for the menagerie of the prince of Conti, two wild goats which he had reared, they followed him merely through their attachment to him. When he arrived near the town of Beſançon, they were frightened by a herd of cows, immediately betook themſelves to flight, climbed up the ſteepeſt rocks in the neighbourhood, and abandoned their guide, who was under great embarraſſment: he, however, went after them, and, having called to them, they ſoon returned, and followed him as before.

I thought it neceſſary to enter into theſe details, to deſtroy that prejudice which repreſents this animal as wild and untractable. What I have ſaid proves, on the contrary, the docility, familiarity, and attachment, of which it is ſuſceptible.

[To be concluded in next Number.]

XXIX. *Ac-*

XXIX. *Account of the Results of ſome comparative Experiments made with the Teylerian Electrical Apparatus and Volta's Metallic Pile. By* VAN MARUM *and* PFAFF. *Communicated to Profeſſor* VOLTA *in a Letter dated November* 1801. *Abridged by* LUKE HOWARD, *Eſq. from* Journal de Chimie *by J. B. Van Mons* *.

A PILE of 200 pairs of metals was conſtructed in the uſual method of Volta. The metals employed were Dutch three florin pieces of nearly $1\frac{1}{2}$ inch in diameter, and correſpondent pieces of zinc. The firſt experiments were directed to charging the jars of a large battery containing $5\frac{1}{2}$ ſquare feet of coated ſurface a-piece. A ſingle jar was firſt charged, afterwards different numbers connected, and ſucceſſively augmented to 25. In ſithe rſt, and in each ſucceeding experiment, the charge was effected by mere contact for as little time as poſſible with the pile. The diſcharge in every caſe produced a divergence of the gold leaves in Bennet's electrometer, amounting to $\frac{5}{8}$ths of an inch, which was alſo found to be the degree of intenſity (*tenſion*) of the pile itſelf.

The whole coated ſurface of $137\frac{1}{2}$ ſquare feet was next charged, at ſeveral times, by a pile augmented at each charge. With 40 pairs of metals, the divergence of the electrometer was perceptible. The increaſed divergence cauſed by the pile itſelf at each addition, was equally producible by the battery when charged by it. No difference was perceived in the effects of the negative and poſitive ſtates of the battery in this reſpect.

The ſenſations producible by the diſcharges under the laſt-mentioned arrangements were as follow:

With 20 pairs of metals, the paſſage of electricity was ſenſibly felt in both hands, each being wetted, and graſping a copper conductor of 2 inches diameter. Sixty pairs affected the elbows. With 200, ſtrong ſhocks were received, extending to the ſhoulders. They concluded that the force of the battery in this reſpect was in all caſes only half that of the pile; which indeed was to be expected.

They next proceeded to a comparative examination of the charge communicable by momentary contact of the battery with the conductor of an electrical glaſs plate apparatus of 31 inches diameter. Precautions were uſed to ſecure the conductor from receiving more than a momentary charge from the plate. It was found, by repeated trials, that ſix

* Communicated by Mr. Howard.

repetitions of the contact were required to bring the charge of the battery to that degree of intenſity which the pile communicated at one.

They were of opinion that the ſenſation of the diſcharge differed nothing at equal intenſities, whether the battery were charged by the pile or the plate apparatus. In ſhort, they conſider the two as mere exciters of the ſame electricity; and conſequently, that the ſuppoſed galvanic fluid has no exiſtence diſtinct therefrom.

Van Marum obſerves, that the rapidity of the current of electricity in the pile muſt be inconceivable, ſince, by a contact laſting only 1-20th of a ſecond, it will charge completely 137 ſquare feet of ſurface; an effect exceeding, in his opinion, the power of every known electrical apparatus, except the large one in the Teylerian muſæum!

This pile was very carefully *inſulated* on a plate of gum lac, and ſupported in its vertical poſition by ſticks of ſealing-wax. Cloth ſoaked in ſolution of muriate of ammoniac was uſed to ſeparate the pairs of metals.

They conſtructed a ſecond pile with 5-inch ſquare plates of copper and zinc. This proved much more powerful when arranged in 32 pairs perpendicularly, than when, by laying down four pieces in contact at once, the height was reduced to eight pairs, and the ſurface in each quadrupled. The former pile melted five inches of No. 16. (iron wire) into globules, and reddened ſeven inches. In attempting to carry it higher, the weight of the ſuperior part, expreſſing the liquid from the inferior, deſtroyed the effect. They remedied this by connecting four ſhort piles, and thus obtained the joint action of 110 pairs of the latter metals.

In order to determine whether there exiſts any perceptible difference between the ſpark given by the pile to any conducting body, and that which it receives therefrom, "we employed (ſays Van Marum) a bowl of quickſilver ſuitably connected by a wire with the ſuperior plate of one of the outſide piles, and we brought to its ſurface one while the point of a needle attached to the wire of communication, then the blunter point of the latter. We next connected the quickſilver with the other extremity of the piles, and, repeating this ſeveral times, we could ſee, in one inſtance, the ſpark paſs from the conducting wire into the quickſilver, in the other from the quickſilver to the wire." The reſult ſatisfied them, that there is not the leaſt perceptible difference between the poſitive and the negative ſparks: but this trouble was compenſated by the view of a very beautiful and intereſting phænomenon. "When we touched (ſays Van Marum) the

ſurface of the quickſilver with the end of the fine wire, the combuſtion was ſo ſtrong, that it diſperſed on all ſides ſparkles which had the appearance of thouſands of rays, conſtituting very fine ſuns of ſeveral inches diameter. By ſlowly lowering the wire as it conſumes, this ſpectacle may be continued at pleaſure.

The No. 16. wire, of $\frac{1}{240}$ inch diameter, ſucceeds moſt certainly; but wire of $\frac{1}{150}$ inch makes a more brilliant ſhow. When it is too thick to burn, the ſparks form ſpots of oxide on the bright ſurface of the quickſilver.

The end of a platina wire, of about $\frac{1}{175}$ inch diameter, was melted into a globule by the diſcharge. The ſparks given by the wire of communication, when the latter was large enough, exceeded $\frac{1}{10}$ inch diameter.

They thought much depended on a proper degree of humidity in the ſubſtances interpoſed between the metals; and they adviſe a *cold* ſaturated ſolution of muriate of ammoniac to be uſed, in preference, for this purpoſe.

The intenſities of two columns containing an equal number of plates appeared equal by the electrometer, although their diameters were ſo different as $1\frac{1}{2}$ and 5 inches. On taking ſeveral repeated ſhocks from each, their powers in this reſpect ſeemed alſo to be alike. In the fuſion of wire, however, the larger diameter had an evident advantage; which was attributed to the greater velocity of the current of electricity flowing through the metal.

By diminiſhing the diameter of the humid conductors between the plates, the force of the pile was leſſened in proportion. On ſubſtituting water, or ſolution of ſea ſalt, to that of ſal-ammoniac, the ſame pile which before melted four inches of wire could not be made to redden a ſingle line. Whence ariſes the ſuperior effect of the ammoniacal ſalt? Is it becauſe it oxidates the metals more? It appeared, indeed, that the action of this pile diminiſhed in proportion as the oxidation of the metals did ſo: but, on the other hand, from the reſult of ſeveral comparative experiments with the ſulphuric, nitric, and muriatic acids, they conclude that the prompt oxidation produced by theſe means is ſo far from increaſing, that it rather leſſens the power of the pile in all reſpects. Neither did ammoniac, which was alſo tried ſeparately, produce an effect at all comparable to that of the neutral ſalt *. The oxidation of the metals, therefore, although it

* The author does not ſay whether ſolution of pure ammoniac, or of the carbonate, was uſed. In either caſe, it does not appear to us that it could be applied in ſuch a degree of concentration as to afford a fair compariſon

it muſt be allowed to contribute ſomething to the effect, does not exclude the cooperation of ſome other property which may reſide in the ammoniac. This opinion ſeemed to the authors to receive ſupport from a ſubſequent experiment, in which a ſaturated ſolution of potaſh exhibited a decided ſuperiority over water when both were ſucceſſively uſed in the ſame pile, although it did not at all tarniſh the metals.

Still further to elucidate this point, the authors proceeded to examine the comparative action of a pile in *vacuo* in oxygen and in other gaſes.

Having placed 60 pairs of zinc and ſilver, with a proper metallic communication, under the receiver of an air-pump, and exhauſted the latter to a preſſure of five lines, they found the action of the pile in no way changed. Air was admitted, and again exhauſted, and the pile left in *vacuo* for an hour, the effect ſtill continuing the ſame. Carbonated hydrogen was now admitted into the receiver, and, being afterwards exhauſted, was replaced by azote, without any perceptible variation, in either caſe, from the uſual appearances. Laſtly, having again exhauſted the receiver, it was filled with oxygen gas from manganeſe. The ſhocks now became ſtronger, and the ſparks much larger, brighter, and eaſier to obtain. On exhauſting the oxygen they were ſurpriſed to find the pile now really affected by the vacuum, and acting with much leſs force than in either of the preceding caſes. The atmoſphere of oxygen, and the vacuum after it, being once more repeated, left no doubt as to the reality of this increaſe and diminution of effect; the pile alſo returning nearly to its ordinary ſtrength on being reſtored to the atmoſpheric air.

The letter concludes with ſtating the effect of increaſing the 5-inch ſquare pile to 200 pairs; the diſcharge of which melted 23 inches of wire, and entirely reddened 33 inches: alſo three other experiments, which prove that, in order to augment the power of Volta's pile, it is neceſſary to increaſe the number, but not the ſurface, of the plates it is compoſed of.

of the effect of ammoniac, properly ſpeaking. It is not extraordinary that ſuch diverſity ſhould be found among liquids as among ſolids in their powers of conducting electricity.—H.

XXX. *Copy*

XXX. *Copy of a Letter from* M. DE DREE *, *Brother-in-Law to* M. DE DOLOMIEU, *to Profeſſor* PICTET, *of Geneva* †.

YOU requeſt me to give you ſome particulars reſpecting the valuable friend whom we have juſt now loſt. I ſhall ſatisfy you with pleaſure, for however afflicting may be the recollections which ſo recent a loſs excites every moment, I experience a kind of ſatisfaction by communicating them to a friend filled with the ſame ſentiments as myſelf for this worthy man.

Yes, Sir, it was at the moment when he enjoyed, in the higheſt degree, the tender affection of all his friends, when he called to mind, with pleaſure, the particular marks of it which he had received at Geneva; and when he felt, in the livelieſt manner, thoſe teſtimonies of reſpect of which he had been, and ſtill was, the object, that he was ſuddenly ſnatched from his numerous friends. It was at the moment when he ſeemed to have acquired a new zeal for geology, that ſcience for which Nature makes us pay very dear, ſince ſhe does not ſuffer her receſſes to be explored but at the expenſe of the lives of thoſe who have the courage to force their way to her. You can already reckon two friends among the number of her victims ‡. It was at the moment when he was preparing to undertake a journey which, as he told me, was to complete his knowledge, and enable him to render his labours and reſearches uſeful; in a word, at a moment when his exiſtence became more and more intereſting to the ſciences, which he promoted with as much attachment as poſſible. He has left very numerous notes reſpecting his travels and various facts he had obſerved; together with ſome manuſcripts on mineralogy, the fruits of his meditations and captivity. But theſe works, however intereſting, had need of being arranged and illuſtrated by the ſame ſpirit which collected the obſervations.

During the three weeks he remained with us, we every day made a lithologic tour. I had diſcovered to him a kind of treaſure in this way, which is found in a ſmall circumference around my habitation, and which preſents ſome very

* M. de Drée is a very intelligent amateur of mineralogy, who poſſeſſes, at Paris, one of the beſt collections of this kind belonging to an individual. He married a ſiſter of Dolomieu; and it was in his houſe at Chateauneuf, in the department of Saone, that this celebrated naturaliſt breathed his laſt.—P.

† Communicated by profeſſor Pictet.

‡ M. de Sauſſure and Dolomieu.

intereſting geological facts; and he had made large collections for his friends. He poſſeſſed an excellent ſtate of health, and we both enjoyed the ſtrongeſt reciprocal friendſhip. He was cheerful, and foreſaw no other chagrin than that of his ſpeedy departure to Paris, when he was attacked by that dreadful malady, of which he had ſo little expectation, that, on the forenoon of the day on which he fell ill, he aſked me where we ſhould go a-mineralizing in the afternoon. The violence of the fever firſt attacked the brain; and we made haſte to call in the aſſiſtance of art, but, unfortunately, without ſucceſs. The firſt period of the malady produced agitation and pain: his head was ſometimes confuſed. The ſecond period, which may be dated from the 4th day, was marked with an almoſt continual dejection, which always increaſed till the laſt moment. During the laſt period he experienced no pain; he found himſelf eaſy; and an hour before he reſigned his tender and ſenſible ſoul into the hands of his God, he ſtill ſaid he was well. In a word, he ceaſed to live by an inſenſible gradation, and like a lamp which becomes extinguiſhed for want of aliment.

However, after he was attacked by the diſeaſe he foreſaw the danger which threatened him; and he beheld his laſt moment with that coolneſs and tranquillity which belong to great minds, like his own. He was much more in pain for the grief he was about to occaſion to his ſiſter, than for his own ſituation; and the only object of regret which ſeemed to affect him in his laſt moments was that of not being able to bid adieu to all his friends. You, Sir, were among the number of thoſe for whom he had the greateſt affection; and he no doubt teſtified to you the regret he experienced on hearing that you was not to paſs this way on your return from England.

If the courſe of his life has been tempeſtuous and agitated, by troubles occaſioned to him in conſequence of that jealouſy which always attends merit, his laſt moments formed a ſtriking contraſt with that agitation; for he gave vent to no complaint againſt thoſe who, by the moſt rigorous treatment, had attacked in him the principle of exiſtence; and his ſoul ſeems to have taken its flight from the world with calmneſs and ſatisfaction, and as if it were ſmiling at the picture of life which it ſurveyed at the moment of its departure.

Madam de Drée and myſelf learned, with great ſenſibility, the proof of attachment which you as well as ſome of your countrymen have given to the memory of Dolomieu, in requeſting to be at the expenſe of having a portrait of that celebrated man engraved from the excellent painting in the poſ-

ſeſſion

feſſion of La Metherie. This idea occurred alſo to that friend, and was at the ſame time my own. I have written to him, that, your wiſh in this reſpect having been the firſt expreſſed, we are far from oppoſing this impulſe of your hearts, ſo flattering to the memory of our common friend.

In regard to myſelf, I expect, with the aid of that portrait, and another very like in the poſſeſſion of his ſiſter, to have a buſt formed, which I ſhall have the honour of ſending you as a mark of my acknowledgement for the friendſhip you teſtified towards him. And as his dear remains reſt with me, the laſt granite we broke together is deſtined to be a tomb, which will ſhow, till the lateſt periods, the ſpot where he employed himſelf in his laſt mineralogical labours.

XXXI. *Notices relative to the Sciences and Arts in France.*

THE muſæum, the gallery of antiques, and that of Apollo, in Paris, are open to public view on the 8th, 9th, and 10th days of each decade. The other days are deſtined for the ſtudents. Foreigners, however, are admitted daily from ten in the morning to four in the afternoon, on preſenting their paſſports; and this indulgence is extended to all perſons arriving from the provinces, and who are provided with paſſports. Theſe exhibitions are gratuitous.

The gallery of Apollo contains the drawings which belonged to the late king. Five hundred of them, framed and glazed, are exhibited at a time. The collection conſiſts of about twelve thouſand. Among theſe are thirty by Raphael, and a conſiderable number by Julio Romano, Titian, Paul Veroneſe, Carachi, and in general by all the great maſters of the three ſchools.

Such in Paris is the quantity of pictures brought from every part of France and Italy, from Flanders, Brabant, Germany, Piedmont, &c. &c. that, to get rid of a part of them, fifteen new muſæums are now eſtabliſhing in as many principal cities of France. In proportion as the buildings prepared for them are ready for their reception, the paintings, drawings, ſtatues, &c. are forwarded.

The pictures in the grand muſæum in Paris conſiſt, one half of the Italian ſchool, one fourth of the Flemiſh, and one fourth of the French. Beſides this, there is a muſæum of the French ſchool in the Palace at Verſailles. Twenty of the apartments in the palace are dedicated to this purpoſe. In addition to the pictures of the French ſchool, there are many others by the beſt maſters of the Italian and Flemiſh ſchools.

This muſæum is open every day. The cielings of three new apartments, to be added to the galleries of antiques, are now painting. One of them is deſtined for the reception of Egyptian antiquities.—In this muſæum will ſhortly be exhibited the tomb of the muſes, ſeveral antique altars and chandeliers, two ſuperb ſtatues by Michael Angelo, one by Puget (Mars repoſing), and ſeveral ſtatues of the French ſchool.

There is ſtill a French academy at Rome. Three prizes for painting, ſculpture, and architecture are awarded yearly; and the three ſucceſsful candidates are ſent to Rome to remain there four years at their government's expenſe. By this regulation there are conſtantly twelve French ſtudents at Rome, each of whom is obliged, in the courſe of his ſtay there, to ſend home one production at leaſt. Theſe productions form a part of the muſæum of the French ſchool at Verſailles.

The Paris exhibition of the works of living painters, ſculptors, and architects, is opened yearly in the months of September and October.

The national library in Paris, comprehending the cabinet of medals and antiques, is open to public view on the 3d, 6th, and 9th days of each decade from ten till two o'clock. The other days, with the exception of that of the decade, are ſet aſide for the ſtudents. The library conſiſts of about ninety thouſand volumes, and upwards of eighteen thouſand manuſcripts, in all languages. There is, beſides, a library of engraved antient and modern muſic.

The collection of medals is immenſely rich. In the year 1800, the entire collection belonging to Pius VI. was added, together with a great part of his gems, the Iſiac table, and ſeveral Egyptian ſtatues in bronze, baſaltes, granite, &c. Here are given annually and gratuitouſly, courſes of public lectures on the Oriental and European languages, chemiſtry, natural hiſtory, botany, antiquities, mythology, &c. by the moſt celebrated profeſſors.

The *Hotel des Monnoyes* (mint) contains a very fine cabinet of mineralogy. Public lectures on that ſcience are given every other day, and the cabinet is daily open to the public.

The garden of plants is alſo open daily. During the ſummer, courſes of practical botany are given by the moſt noted profeſſors.—The cabinet of natural hiſtory is one of the compleſeſt in Europe. It is open to the public five days in the decade: the intermediate days are for ſtudy, and for the courſes of public lectures.

A great collection of antiquities is expected in Paris from Italy. Upwards of five hundred caſes filled with antiques

of

of every deſcription, which had been ſeized by the Neapolitans on the banks of the Tiber, are to be reſtored to the French by virtue of the laſt article of the treaty of peace with the king of Naples: for this purpoſe two commiſſaries have been ſent to Italy; one of them the ſon of Chaptal the miniſter for the home department, the other Dufourny, an architect.

The inſtitute for deaf and dumb perſons, conducted by Sicard, is perhaps one of the moſt uſeful eſtabliſhments which Paris can boaſt. The progreſs made by the pupils under their able and benevolent profeſſor excites an univerſal admiration. Maſſieu, the favourite of the abbé Sicard, and the moſt intelligent of his pupils, acts as ſecond maſter. Some time ago a lady aſked him in writing what was *gratitude*: to which he replied without heſitation, *The memory of the heart.* It was impoſſible for any one of the ſpectators, gifted with all his ſenſes, to have replied with more juſtneſs, truth, and delicacy.

The manufacture of porcelain at Seves, near Paris, is open daily to public view; as is alſo that of the Gobelins in Paris. In the latter, a number of artiſts are now employed in copying in tapeſtry the moſt pleaſing ſubjects which can be ſelected;—landſcapes, cattle pieces, pieces of fruit, flowers, &c. Some of theſe are intended for preſents; the others are ſold in proportion as they are finiſhed.

The national inſtitute of muſic, which was eſtabliſhed in 1793, and which is now ſtyled the conſervatory of muſic, is under the adminiſtration of five perſons, who are lodged in the building, each with a ſalary of five thouſand livres. The profeſſors, ſeventy in number, have each a ſalary of fifteen hundred livres. The number of pupils, males and females, amounts to about five hundred. The prizes are diſtributed annually with great pomp; and the ſucceſsful candidates ſing or play publicly the pieces which have been crowned by ſucceſs. From ſix to eight concerts are given annually, to excite an emulation in the pupils, and to diſplay their talents.

XXXII. *Ob-*

XXXII. *Observations on Maddering; together with a simple and certain Process for obtaining, with great Beauty and Fixity, that Colour known under the name of the Turkey or Adrianople Red.* By J. M. HAUSSMANN*.

I HAVE already indicated, in the *Annales de Chimie*† and the *Journal de Physique*, that earths and metallic oxides have more or less the property of attracting and retaining the colouring parts of vegetable and animal substances; alumine and the oxide of iron possess it in a greater degree than the oxide of tin; but the attractive force of the latter far surpasses that of the other earths and metallic oxides in regard to the colouring parts of the said substances.

Alumine and metallic oxides do not retain, with the same force of adhesion, the colouring parts of all animal and vegetable substances indiscriminately; that of madder adheres much stronger than those of the other colouring substances, which may be classed in the following order: kermes, cochineal, logwood, yellow India wood, woad, quercitron, Brazil wood, red India wood, yellow berries, &c. The gall-nut, shumac, and other astringent colouring substances, act principally by means of the gallic acid, and, in regard to their degree of fixity, may be placed immediately after madder: the case is not the same with the Prussic acid, which communicates a colour to different metallic oxides, from which it can be separated cold by alkaline leys.

To judge of the fixity of colours arising from animal and vegetable substances, the best method is to employ a ley of oxygenated muriate of potash or soda, with excess of alkaline carbonate. The longer or shorter resistance which the colours make in this ley, will indicate what they will make when acid, alkaline, saponaceous, and other reagents are employed.

In the art of dyeing, and that of cotton-printing, the name of maddering is given to that process by which the colouring parts of madder are transferred, by means of water with the aid of heat, to alumine, or to the oxide of iron fixed in any kind of stuff.

* From the *Annales de Chimie*, No. 122.

† We must here mention, that C. Chaptal, minister of the interior, a good judge in matters of this kind, when he communicated to us these observations, wrote as follows: "C. Haussmann, manufacturer of printed cottons at Laglebach near Colmar, in the department of the Upper Rhine, well known among those chemists who apply the discoveries of science to improvements in the arts, transmitted to me the annexed memoir. In my opinion it will be of utility to make it known in your Annals, and the author on my request has consented to its being published." *Note of the Editors of the* Annales de Chimie.

The

The brightneſs and fixity of the colours obtained from maddering depend not only on the proceſs, but alſo on the ſtate and purity of the water as well as of the madder. It is therefore abſolutely neceſſary to avoid or to render inactive every acid, alkaline or ſaline ſubſtance that may be contained in the water, or in the madder itſelf. I have ſhown that, by adding carbonate of lime, (pounded chalk,) madder which I ſuſpected to contain gallic acid was corrected; but that my friend Charles Bertholdi, profeſſor in the central ſchool of the Upper Rhine, afterwards found that it was ſulphuric acid united to magneſia.

The important diſcovery of this addition of chalk, which I made twenty-five years ago, has given birth to many manufactories, and improved all thoſe eſtabliſhed near waters which do not run over or hold in ſolution this earthy ſalt, without which it is abſolutely impoſſible to obtain beautiful and fixed madder colours. This chalk ſince that time has become a new object of commerce; and as the price is very moderate, I have not yet determined the juſt proportion to be employed: in general, I take one part for four, five, or ſix, of madder.

In order to obtain the brighteſt madder colours, it is not ſufficient to attend to the quality of the water and of the madder: it is neceſſary alſo to obſerve the degree of the heat of the bath: a low temperature will check the attraction of the colouring parts, and prevent them from being extracted, while one too high will favour the adheſion of the yellow particles of the madder, which obſcure and tarniſh the ſhades intended to be produced. The only colour which gains by increaſing the heat is black. I have always obſerved, that on withdrawing the fire from below the boilers, when the hand can no longer be held in the aqueous vehicle which they contain, if the maddering be then continued for two or three hours, the moſt ſatisfactory reſults will be obtained, as the furnace ſtill retains a ſufficient quantity of heat to maintain the vehicle at the ſame temperature, eſpecially when, according to cuſtom, large boilers are employed. Beſides, it would be very difficult to fix a determinate degree of heat by the thermometer when the furnaces are large.

The yellow parts of the madder as well as of other colouring ſubſtances are, it is probable, nothing elſe than the colouring parts themſelves combined with oxygen. The product of this combination, by acquiring greater ſolubility, ſuffers itſelf with more difficulty to be taken away by clearing, if the heat has not been properly regulated during the proceſs of dyeing. I have often obſerved that madder and other colouring

louring ſubſtances, when long expoſed to the atmoſpheric air, do not give colours of the ſame intenſity and the ſame brightneſs as before; either becauſe theſe ſubſtances abſorb the oxygen of the atmoſphere, or that they procure this radical from the water which they attract, or which they naturally contain as a conſtituent principle, and which is decompoſed by a ſlow and inſenſible fermentation. The expoſure, on the graſs, of cotton or linen dyed a dark madder red, might ſupport the idea of a change to a reddiſh yellow; for this dark colour becomes clearer but fainter by the expoſure, and then aſſumes a more agreeable ſhade of crimſon. I have ſhown, in a memoir on indigo, inſerted in the *Journal de Phyſique* for the year 1788, that nitric acid changes this blue fecula into a yellowiſh ſubſtance: a ſimilar change takes place by expoſing, on the meadow, the ſame fecula fixed on any ſtuff whatever; and the yellow reſulting in theſe two ways is more ſoluble in warm water than in the ſame liquid when cold. It however appears that the combination of oxygen is not the only cauſe of the change of colours, ſince curtains of any ſtuff dyed or coloured any ſhade whatever by vegetable or animal ſubſtances, and expoſed to the light, loſe their colour entirely in the courſe of time on the ſide expoſed to the ſolar rays, while the oppoſite ſide retains it for a conſiderable time. If the rays of the ſun then give more vigour to living bodies of the animal and vegetable kingdom by diſengaging from the latter oxygen gas, it appears that they act with deſtructive influence on the ſame bodies deprived of life, by decompoſing their conſtituent principles. In all caſes it will be proper to preſerve the colouring ingredients in dry places ſheltered from the light, which acts upon theſe bodies perhaps only by decompoſing the conſtituent aqueous part, the oxygen of which may join the carbon to form carbonic acid. Reſinous and oily ſubſtances ſhould be preſerved in the ſame way. Theſe conjectures prove at leaſt that the action of the ſun's rays, or of light, on theſe bodies, in general preſents a vaſt field for intereſting experiments to be undertaken.

If in maddering brighter colours are obtained by carefully regulating the heat, a ſacrifice is made at the ſame time of a ſmall portion of the colouring parts of the madder, which cannot be entirely exhauſted except by then increaſing the heat to ebullition; but as the colours thus obtained are degraded more or leſs in the ratio of the quantity of the madder, the gall-nut or ſhumac uſed, this method muſt be employed with caution, and principally for common effects, either in regard to cotton or linen. To avoid as much as poſſible the loſs of madder after the maddering of good articles

cles has been terminated, and before the common ones are put into the boiler, powdered gall-nut or ſhumac muſt be added, with a new but ſmall portion of madder: the proceſs muſt be managed alſo in ſuch a manner, that the ebullition ſhall not take place till two hours after.

I ſeveral times tried to exhauſt the madder by ſimple ebullition, and without adding any thing elſe than chalk; but I found that this was unfavourable to all colours, black excepted: it even appeared that the effect of the madder was much leſs than when the heat was moderately applied, and when the accumulated caloric eaſily decompoſed the colouring ſubſtance. It is this tendency to be decompoſed, and particularly by fermentation, however little it be moiſtened or diluted with water, which has hitherto prevented me from obtaining a ſubſtantial colour, pretty dark, and ſufficiently fixed to be applied on any kind of ſtuff. I obſerved alſo, that if the heat was carried too far the firſt time, in circumſtances when it was propoſed to madder a ſecond and third time, it prevented me not only from obtaining bright and agreeable ſhades, but alſo of the requiſite intenſity. The aqueous vehicle of the madder, at too high a temperature, never fails to weaken the adheſive force of the alumine and the oxide of iron to the ſtuff, and to take from it a portion, which an experienced eye may eaſily remark on examining the bath.

I ſhall here repeat, that for common and low-priced articles it is indiſpenſably neceſſary to employ gall-nuts or ſhumac, which will ſave one half and even two thirds of the madder; but the colours obtained are neither ſo fixed nor ſo bright. The addition of chalk, however, muſt not be omitted; otherwiſe the gallic acid will carry away a portion of the alumine and coloured oxide of iron, which will weaken the ſhades, and, by tarniſhing the ſtuffs, will alſo attack the white which may have been preſerved in them. Without the addition of gall-nuts or ſhumac, it ſeemed to me impoſſible to exhauſt the madder entirely of its colouring parts; which made me preſume that their adheſion is favoured by the viſcid nature of the tanning principle of theſe aſtringent ſubſtances, which carry away and combine with themſelves the colouring parts. I ſhall obſerve alſo, that gall-nuts as well as ſhumac loſe the property of dyeing black; and acquire, on the other hand, that of dyeing or colouring alumine yellow, oxide of iron olive green, by the addition of chalk, the calcareous baſe of which unites itſelf to the gallic acid. Do theſe yellow and olive-green colours ariſe from any peculiar ſubſtance contained in the gall-nuts and ſhumac, or are they indebted for

their origin to the tanning principle? This remains to be examined.

The quantity of madder to be employed in dyeing ought not only to be proportioned to the extent of the ſurfaces to be maddered, but alſo to the concentration of the liquors of the *acetite of alumine and iron*, improperly called mordants; that is to ſay, to the greater or leſs quantity of alumine and oxide of iron which theſe ſaline liquors, either inſulated or mixed together, when they dry on the articles to be dyed, may have left or depoſited there by the evaporation of the acetic acid. If the objects to be dyed are not numerous, and, in particular, when bright ſhades only are to be produced, they may be maddered only once; but when they are numerous, and intended to have dark ſhades, the maddering muſt be repeated twice, and even thrice. Three quarters of a pound of madder of a good quality are ſufficient for dyeing a piece of white Indian cloth of ten ells in length and three quarters broad, intended to exhibit only a few coloured objects: the quantity of the colouring ſubſtance muſt be increaſed in the ratio of the maſs of alumine and oxide of iron, fixed on a piece of ſtuff of the above dimenſions. It may be extended to 6, 8, 10, and even 12 pounds, for a ground well covered with a lively and very intenſe colour. Intelligence and practice in the management of a dye-houſe will not fail to indicate nearly the proper proportions.

Whatever care may be employed in maddering to avoid the adheſion of the yellow parts, the colours obtained will be far from having all the beauty and fixity which they might acquire by clearing, preceded by very long ebullition in exceedingly pure water. This ebullition alone, by the addition of bran, will ſerve to brighten the colour: more roſy reds will be obtained by employing ſoap with or without the addition of bran; carbonate of potaſh or of ſoda, ſubſtituted for bran, will make the reds incline to crimſon; but I muſt obſerve, that unleſs the workman chooſes to run the riſk of making the reds entirely brown, and in ſuch a manner that it will not be poſſible to reſtore them, it will be neceſſary, before ſoap and alkalies are applied to the ſtuffs, to expoſe them to the action of the ſtrongeſt heat that can be communicated to water. This operation will be attended with ſucceſs, if as little paſſage as poſſible be afforded to the ſteam, and if the boilers employed be converted into a ſort of digeſters. The fixity of the colours will be proportioned to the time employed in expoſing them to the action of the boiling water. It is needleſs to obſerve, that there is no

danger

danger of spoiling the colours by soap and alkaline carbonates, when the maddering, instead of being directed with a moderate heat, has been carried to ebullition, as is practised in many dye-houses; but, in this case, the colours obtained are more difficult to be cleared.

As water charged with oxygenated muriatic acid easily carries away the colouring parts of madder, as well as other vegetable and animal substances, by decomposing them; and as acids more concentrated may, in their turn, take from the stuffs the colourless alumine and the oxide of iron, it is impossible for me to adopt the idea of a chemical combination of the colouring parts with alumine and metallic oxides, which, in my opinion, when fixed and coloured on any stuff, form only compound aggregates.

The clearing of objects printed on a white ground requires modifications, which I shall detail on a future occasion, when I find leisure. It will therefore be sufficient at present to state, that after continuing for some time my experiments on the Turkey red, inserted in the *Annales de Chimie* for the year 1792. I at last found a red much more beautiful and durable than that of the Levant, by fixing alumine on cotton, thread, and linen, by an alkaline solution of this earth mixed with linseed oil. The following is the process I employed.

[To be continued.]

XXXIII. *Notices respecting New Books.*

Ausführliche Geschichte der Theoretisch Praktischen Uhrmacherkunst, &c. A History of Clock- and Watch-making, both Theoretical and Practical, since the earliest Method of dividing the Day to the End of the 18th Century. By I. H. Moriz Poppe, 1801. 8vo. 564. p. 8.

THE author of this work having published, in 1797, An Essay towards a History of the Origin and Progress of Clock- and Watch-making, consisting of six sheets; it met with such a favourable reception, that he was induced to improve and enlarge it to its present size. He acknowledges the obligations he is under to professors Kastner and Beckmann, who assisted him with their advice and information; and who, on account of their learning and extensive reading, were able to supply him with much useful information. The whole work is divided into ten chapters.

I. *The oldest method of dividing the day, and the invention of sun-dials.*—The period when sun-dials were invented is as little

little known as the country where, and the perſon by whom. The author, however, has collected from the oldeſt authors what is to be found on the ſubject, and examined it with critical accuracy. The earlieſt account of inſtruments deſtined to mark the lapſe of time by the ſun's ſhadow is to be found in the Bible and in Homer. A ſun dial conſtructed according to the deſcription of Vitruvius was found in the year 1741, among the ruins of a villa on the Tuſculan hill at Rome. A ſun-dial of the ſame kind is ſtill to be ſeen at Athens, ſtanding on the ſummit of a rock, to the right of an edifice built by Thraſyllus.

II. *Uſe of ſun-dials, and the diviſion of the day into hours, among different antient nations; gradual improvement of the art of conſtructing and uſing them to the preſent period.*—The Indians, Siameſe, Tartars, Perſians, Chaldeans, Egyptians, and Chineſe divided the day into 60 hours, and each hour into 60 minutes, &c. Theſe people even employed a gnomon for the purpoſe of placing their temples according to the principal quarters of the globe. We know from hiſtory that the Chineſe made uſe of gnomons 12 or 15 centuries perhaps before the birth of Chriſt. The old geographers, by examining the length of the ſhadows of gnomons of equal height, determined the latitude of places, and the obliquity of the ecliptic. Beroſus brought to Greece from Aſia the diviſion of the day into twelve hours; and the firſt ſun-dial. Anaximander, about ſix centuries before Chriſt, made an improvement in ſun-dials, as did alſo Anaximenes. Eudoxus, four centuries before Chriſt, conſtructed a ſtill more perfect ſun-dial, under the name of *arachne*. Apollonius of Perga, about a century before Chriſt, invented the *pharetra*. Patrocles found out the *pelekinon*, Dionyſiodorus the ſun-dial in the form of a cone, Cleanthes the hemiſphere or *ſcapha*, Parmenio the *proſtapiſtorumena*, Theodoſius and Andreas the *proſpanklima*. Vitruvius makes mention of three other ſun-dials; *gonarcha*, *engenaton*, and *antiboreum*, without deſcribing them or naming the inventors. The author deſcribes all theſe ſun-dials with as much preciſion as the accounts of them remaining will allow, and with literary and technical accuracy. There were public ſun-dials both at Athens and Sparta. Eratoſthenes and Archimedes employed dials very accurately divided for aſtronomical purpoſes; and all the cities of Greece of any conſideration ſoon had public inſtruments of this kind. Ring-dials ſoon began alſo to be uſed. A dial of this kind was ſuſpended in the large ſhip of Hiero; but the Grecian navigators, for meaſuring the ſtate of the ſun, the time of the day, and the ſtars above the horizon, employed rather the hodometer

deſcribed

deſcribed by Vitruvius, which ſeems to have furniſhed the firſt hint for our meaſurers of time, conſtructed with wheel-work. It was very late before the Romans had real ſun-dials. They employed in their ſtead obeliſks, to which good gnomons were applied. The largeſt of all the gnomons of modern times was that of Ulug Beigh, erected at Conſtantinople in the 15th century, which was 183 feet in height. The Bononian obeliſk, 83 feet high, erected by Caſſini, that of Paris, and the one which Pope Clement XI. cauſed to be conſtructed, are alſo celebrated. In the antient gnomons, the hours were indicated by the ſhadow of a ſtyle; in the modern ones the ſame thing is done by the ſun's rays paſſing through a hole. The Greeks and Romans employed perſons of both ſexes to announce to them the hours, as indicated by the public gnomons. Trimalchio cauſed the hours to be announced to him by a trumpeter. This was afterwards cuſtomary at the temples. It is not known, however, when portable ſun-dials came into uſe. Some of theſe inſtruments, after being buried more than 1500 years, were found between 1730 and 1740 in the territories of Rome: of theſe remains of antiquity the author gives a deſcription. That Purbach, an aſtronomer of Vienna, introduced ſun-dials into Germany about 300 years ago is not probable, as the Germans, no doubt, obtained them by their intercourſe with the Romans. In the 16th century artiſts took great pains to conſtruct ſun-dials in a great many ingenious ways; about the ſame period lunar and aſtral dials were invented. There is a ſun-dial at Beſançon which is ſeen only when the ſun ſhines. This is alſo deſcribed, as well as the dial below the roof of the council-houſe at Ingoldſtadt. Another at Alençon, ſtill more ingenious, is connected with wheel-work, and indicates true and mean time. In the muſæum at Göttingen there is a collection of ſun-dials, and other works of art of a ſimilar kind, preſerved in a box.

III. *Oldeſt method of dividing the night. Invention of water-clocks and ſand-glaſſes, and the progreſſive improvement of them to the preſent time.*—The circumſtance of ſun-dials being of no uſe in the night-time, and during cloudy weather, gave occaſion to the invention and improvement of water-clocks; traces of which may be found among the oldeſt nations, the Chaldeans and Egyptians. They were in uſe alſo at an early period among the Chineſe. They were employed by the Egyptian aſtronomers for meaſuring the diameter of the ſun. In their firſt ſtate they were called *clepſydræ*, becauſe the water iſſued from them drop by drop. The defect, in regard to the inequality in the efflux was at firſt remedied by the ſmall ſtick of the Indians having a hole bored in it,

and which floats ſome time on the water, but which after a certain period, diſcovered by repeated trial, becomes immerſed. It was afterwards contrived to keep the water-cloek always full, by ſuffering the ſame quantity of water to run in as that which flowed out, that equal portions of time might be indicated by an equal fall. A funnel or inverted pyramid was then employed, in which the water deſcended in unequal parts, but through equal degrees marked out on a ſcale. In the courſe of time the knowledge of aſtronomy was applied to theſe clocks, and ſome were conſtructed in a very ingenious manner: of this kind was the *anaphoricum*, which the author deſcribes; alſo the retrograding cloek, and winter water-clock. Plato brought the firſt water-clock to Greeee. The firſt that ever appeared at Rome was ſhown by Scipio Naſica, about the year 157 before Chriſt: but ſoon after theſe clocks were eommon, not only at Rome, but in other towns of the Roman empire. When Julius Cæſar invaded Britain, he found that the inhabitants had water-clocks. In general they were more prized than ſun-dials. Athenæus conſtructed a clock which indicated the hours by the hiſſing noiſe of the air forced through a narrow hole by the preſſure of water. Together with public ſun-dials, public water-clocks were alſo ſoon introduced. Theſe were eſtabliſhed even in the palaces of the great. That the hours as announeed by theſe water-elocks may be more eaſily made known to the public, the Turks have criers poſted on high towers,—and the Chineſe, perſons who ſtrike on large bells. The Japaneſe employ burning matches to indicate the time, and announce the hours in the ſame manner as the Chineſe. The uſe whieh Hipparchus and Ptolemy made of water-clocks contributed to their improvement: they were much improved by Hero, but during the next ſeven centuries nothing was done in this reſpect. The clocks of Boethius, that which Harun al Raſchid ſent as a preſent to Charlemagne, and that of the philoſopher Leo of Conſtantinople, were mueh celebrated, though the invention of elocks with wheel-work rendered water-elocks unneceſſary. The latter, however, continued ſome time after in uſe; and about the year 1660 water-clocks in the form of a drum, which are thoſe called properly at preſent water-clocks, were invented it is probable in Italy, but rather for curioſity than for real uſe. The obſervation that the water ſoon evaporated, may have given occaſion to ſand being uſed in ſuch elocks inſtead of water. The period when this change was made is not known. There is reaſon only to conjecture that the Egyptians and Chaldeans had hour-glaſſes. In the Monachal Laws the word *klepſammidia* oecurs only in writings of the eighth century; and it was not till a much later

period that attempts were made to conſtruct hour-glaſſes in an elegant and ingenious manner. Many of them are deſcribed by Francis de Lanis, as well as by Schott and Ozanam. Rivault employed ſuch ſand-glaſſes for aſtronomical obſervations; and likewiſe Tycho Brahe, who uſed alſo clocks conſtructed with quickſilver.

IV. *Ingenious water-clocks and other machines, the moving principle of which was different from that of our common clocks that go by wheel-work.*—Under this head the author ſpeaks of the machine which king Gonebaud received as a preſent from Theodoric king of the Goths. He gives alſo a deſcription of the before-mentioned clock ſent by Harun al Raſchid to Charlemagne. At that period the eaſtern princes preſented clocks to the weſtern; at preſent this mode is reverſed. This chapter contains alſo a deſcription of ſome other ingenious clocks from Schott, Kircher, Ozanam, Martinelli and de Lanis; alſo Perrault's pendulum-clock, which was moved by water, and a Chineſe one which Y-Hang cauſed to be conſtructed.

[To be continued.]

Traité Elémentaire du Calcul Differentiel et Integral, &c. An Elementary Treatiſe on the Differential and Integral Calculus, preceded by ſome Reflections on the Method of teaching the Mathematics, and appreciating in Examinations the Knowledge of thoſe who have ſtudied them. By S. F. Lacroix. Paris, An. 10.

THE author of this work is well known by his large treatiſe on the Differential and Integral Calculus, in three vols. quarto, in which he has given a complete view of every thing taught on the ſubject, and which ought to be ſtudied by thoſe who are deſirous of enlarging the boundaries of this ſcience. The time devoted, uſually, to a courſe of analyſis being far from ſufficient to follow with the proper attention ſo many objects, the author found it neceſſary to make a ſelection. This publication, however, is not merely an extract from the above large work; it is a new treatiſe, which cannot fail of intereſting thoſe who are in the poſſeſſion of the other. It is founded on the ſame principles, and will form an excellent introduction to the ſtudy of the more learned works.

Elements of Chemiſtry. By J. Murray, *Lecturer on Chemiſtry, Materia Medica, and Pharmacy.* 2 vols. 8vo. Edinburgh 1801.

THIS work will be found very uſeful to the chemical ſtudent. The author has given a correct view of the preſent

ſtate of the ſcience of chemiſtry, and beſtowed a proper degree of attention on the practical facts and applications, which ſerve to illuſtrate and eſtabliſh its general principles.

A Statiſtical Account of the Population and Cultivation, Produce and Conſumption of England and Wales, compiled from the Accounts laid before the Houſe of Commons and the Reports of the Board of Agriculture; together with Obſervations thereupon, and Hints for the Prevention of a future Scarcity. *By* BENJAMIN PITTS CAPPER. 8vo. Kearſley. 1801.

THE title of the preſent work very well deſcribes its nature, and renders it unneceſſary for us to ſay any thing reſpecting it, further than that it is an intereſting little work. The following ſummary is extracted from it:

The total number of population appears to be		9,500,000
Number of inhabitants in the capital		715,002
Number of pariſhes, churches, and chapels		
in England	10,500	
in Wales	1,100	11,600
Number employed in agriculture		1,737,675
Number as tradeſmen and mechanics	Wales, 55,887 / England 1,840,598	1,896,485
Number of families		1,917,232
Number of perſons to each family		$4\frac{3}{4}$
Number of houſes inhabited		1,598,278
Number of perſons to each inhabited houſe		$5\frac{3}{5}$
Number of houſes uninhabited		58,962
So that there appear buildings ſufficient to contain perſons		430,187
But by making an allowance of 45,000 houſes, that we may ſuppoſe will be occupied by 250,000 perſons, when the army and navy are reduced to a peace eſtabliſhment, the number of uninhabited houſes will then remain		13,962
The number of marriages appear to be annually	1 in	134

We underſtand that Dr. Gruber is at preſent engaged in tranſlating C. Guyton's tract on the means of dis-infecting the air, preventing contagion, and checking its progreſs; which will appear in a few days.

XXXIV. *Pro-*

XXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

THURSDAY, February 25. A letter from Mr. Schröter, of Lilienthal, reſpecting the planet Ceres Ferdinandea, informed the Society that he had obſerved a nebuloſity round the new planet ſomewhat reſembling that of a comet: the diameter of the true diſc being 1·8″, and that of the nebula 2·6″; but the diſtinction was not always equally obſervable. Mr. Schröter conſiders this body as of a hybrid nature, or a medium between a planet and a comet; but he imagines the apparent nebuloſity to be owing to an atmoſphere, and that, according to the different ſtates of this atmoſphere, the light reflected from the planet is either white, blueiſh, or reddiſh.

A table of obſervations of the ſame planet was alſo communicated by Mr. Mechain, through Sir Henry Engleſield.

In the meetings of the 25th of February, 4th and 11th of March, a paper which has been expected for ſome time occupied the attention of the Society; namely, Mr. Howard's, on the analyſis of ſtones that have fallen from the clouds.

Mr. Howard begins with a hiſtorical detail of the various relations of this kind which are found on record, and particularly refers to the eſſays of Mr. King and profeſſor Chladni, and to various authors quoted by them. But the firſt inſtances with which chemiſtry has interfered are thoſe of a ſtone preſented to the French Academy by the abbé Bachelay in 1768; and another examined afterwards by profeſſor Barthold. The ſtones from Sienna in 1794; the large ſtone of 56 lbs. weight which fell in Yorkſhire in 1795, and was exhibited ſoon after in London; and the ſubſtances which fell at Benares in 1798, are the immediate ſubjects of Mr. Howard's inveſtigation. All theſe agree in the general appearance of an aſh-gray ſtony ſubſtance, mixed with ſpangles of pyrites and of native iron, and externally of a dark colour, covered with a ſemi-vitrified and bliſtered cruſt. The abbé Bachelay's was ſuppoſed to contain $8\frac{1}{2}$ ſulphur, 36 iron, and $55\frac{1}{2}$ earth; and ſome of the others were found to conſiſt of ſimilar ingredients. The ſtone which fell near Mr. Topham's houſe in Yorkſhire, penetrated twelve inches deep into the earth, and ſix more into a chalk rock: its fall was accompanied with noiſes like a diſcharge of artillery. A very particular and perfectly authenticated account is given, in the words of Mr. Williams, of ſeveral ſubſtances which fell about

twelve miles from Benares, and penetrated ſome inches into the earth in ſeveral ſpots within the diſtance of 100 yards; their fall being accompanied by a very vivid light.

Mr. Howard proceeds to mention another ſpecimen from the Muſæum Bornianum, now in the poſſeſſion of Mr. Greville, ſaid to have fallen in Bohemia, which agrees with the reſt in its characters. A mineralogical deſcription of theſe ſtones by the Count de Bournon is ſubjoined. They appear to conſiſt principally of ſubſtances of four kinds, beſides the dark cruſt which ſurrounds them: the firſt of theſe ſubſtances is in the form of dark grains, of a conchoidal fracture, from the ſize of a pin's head to that of a pea; the ſecond is a kind of pyrites, the third is metallic iron, and the fourth a gray earthy ſubſtance, ſerving as a cement to the reſt. The proportions of theſe ſubſtances appear to differ in ſome meaſure in the different ſpecimens, the iron abounding moſt in the ſpecimens from Yorkſhire and from Bohemia. Mr. Howard has aſcertained, by a chemical analyſis, that ſilica, iron, magneſia, ſulphur, and nickel, are contained in the different parts of theſe ſubſtances. The globular bodies and the cementing earth each contained about 50 ſilex, 15 magneſia, 34 iron, and $2\frac{1}{2}$ nickel.

From 150 grains of the earthy part of the ſtone from Sienna, Mr. Howard obtained about 70 ſilica, 34 magneſia, 52 oxide of iron, and 3 oxide of nickel; the contents of the ſpecimens from Yorkſhire and from Bohemia were not materially different. Mr. Howard proceeds to inquire into the cauſes of the difference in the reſults of his analyſis and thoſe of the foreign chemiſts, with reſpect to the ſpecies of the earths. After having ſhown the ſtriking analogy between theſe ſubſtances, and their total diſſimilarity to other mineral products, Mr. Howard examines into the form and contents of various ſpecimens of native iron: obſerving that Mr. Prouſt detected nickel in a large maſs of native iron found in South America; Mr. Howard diſcovers a portion of the ſame metal in every ſpecimen that he has examined from different parts of the world. A deſcription of theſe ſpecimens by the Count de Bournon is inſerted, and the large maſs diſcovered by profeſſor Pallas in Siberia, is particularly deſcribed. It is found to contain detached maſſes of ſemi-tranſparent ſubſtances conſiderably reſembling ſome of the conſtituent parts of the ſtones from Benares. Mr. Howard does not give a decided opinion reſpecting the origin of all theſe ſubſtances; he only obſerves that they agree in ſeveral remarkable properties, diſtinguiſhing them from all other bodies, that they all appear, from well authenticated accounts, to have fallen on the earth, attended

tended in moſt inſtances by meteors or lightning, and that it is remarkable that the native iron in all the ſtones contains nickel as well as the other native irons.

A letter was alſo read on the 11th of March from Mr. Von Zach, confirming Mr. Schröter's obſervation of the changeable light of the planet Ceres, which Mr. Von Zach had at firſt attributed to the hazineſs of our own atmoſphere, until he found that MM. Olbers and Schröter were agreed in deriving it from a real change in the light reflected.

On the 18th, an appendix to Mr. Chenevix's paper on oxymuriates and hyperoxymuriates was read; and,

On the 25th, a paper on corundum, by Count Bournon, was begun; but it will take another night's reading to finiſh it. The Count claſſes along with corundum all the varieties of the ruby, ſapphire, amethyſt, emerald, &c. which, from their peculiar hardneſs, are claſſed by jewellers as oriental.

FRENCH NATIONAL INSTITUTE.

Some very intereſting papers were read in the late ſittings, particularly a very learned diſſertation on the ſtructure of primitive mountains, and of that of a part of the Pyrenees; a memoir, no leſs important, on the theory of the moon; and a deſcription of ſome new experiments by C. Coulomb, to prove that all bodies are ſuſceptible of being attracted by the magnet. In the ſitting of March 17th, C. Gerard, engineer of bridges and cauſeways, read a continuation of his very learned diſſertation on the pyramids of Ghizé and Saccara. Chef de brigade Grobert read a memoir on wheels with conical fellies, as uſed in England; and announced an extenſive work on carriages of all kinds. Count de Dieth was preſent at this ſitting.

SOCIETY OF FELIX MERITIS, AMSTERDAM.

The department of deſign in this Society, in the meeting of October 5, 1801, propoſed the following prize ſubjects:

1ſt, A golden medal, of 14 ducats value, for the beſt hiſtorical painting of Abraham entertaining the angels: Geneſis, xviii. 10—12.

2d, A medal, of the ſame value, for the beſt landſcape in the Netherlands.

The paintings to remain the property of the artiſts.

The pieces muſt be tranſmitted to the Society before the 1ſt of Auguſt 1802. The paintings muſt be accompanied with a device, and be ſent, carriage paid, to the preſident of the department of deſign, C. I. C. Linckers, at Amſterdam.

XXXV. *Intelligence and Miſcellaneous Articles.*

VACCINE INOCULATION.

IN our laſt we mentioned that Dr. Sacco, of Milan, had ſent to Dr. Pearſon cow-pock matter taken from the Milaneſe cows. In a work publiſhed by Dr. Sacco, in Italian, entitled, Practical Obſervations on the Uſe of the Cow-pock, as a Preſervative againſt the Small-pox, he gives the following account of the manner in which he procured the pus for inoculation, and alſo a repreſentation of a cow's udder infected with the malady. (See Plate VI.)

"For ſome time I had been extremely deſirous to repeat the experiments of Jenner, and for this purpoſe made diligent ſearch to diſcover the cow-pox in Lombardy, it being extremely difficult, eſpecially in the preſent circumſtances, to obtain the pus from England. A fortunate combination of circumſtances, by which it became neceſſary for me to go to the large town of Vareſe, in the beginning of autumn, procured me an opportunity of examining a number of cows on their way from Switzerland to the fair of Lugano; and by this means I had a favourable opportunity to make ſuch reſearches as might diſcover in ſome one of them the cow-pox. It was on this occaſion, that, converſing with ſome dealers in cattle, and countrymen who had large dairies in Lower Lombardy, I learnt that the cows among us are ſubject to the cow-pox. In this inquiry I took care to propoſe my queſtions in ſuch a manner as to prevent the riſk of being impoſed upon. A farmer of Cremona, who had bought forty cows in Switzerland, and had driven them from thence as far as Vareſe, aſſured me that almoſt all of them had been ſucceſſively attacked with puſtules on the extremity of their nipples, and ſome of theſe were now converted into incruſtations. I viſited the cows, and had an opportunity of verifying his aſſertions. I picked off ſome of theſe incruſtations with an intention of applying them in fomentation, if, perchance, I could not procure the true pus for inoculation. The ſame farmer promiſed me an opportunity of ſeeing this diſeaſe with my own eyes, and for this purpoſe conducted me to a neighbouring meadow, in which we found a herd of cows belonging to a friend of his. We examined theſe cows, and diſcovered on two of them different red ſpots, which the farmer aſſured me was the firſt ſtage of the diſeaſe: no other ſymptom appeared on the cows, but a ſlight degree of dejection. He aſſured me that this was the very diſeaſe

I was

I was in queſt of, and that, in the courſe of two days, the puſtules would unfold themſelves. At this viſit which I made to the cows, there was preſent a dealer in the Griſons cows, who fully confirmed the truth of theſe aſſertions. He alſo added, that in his country he had ſeen the cows afflicted with a ſimilar eruption on their dugs, and to remove the incruſtations it was common to anoint them with boiled oil uſed for varniſh; and that by this means they fell off in the courſe of two or three days. Early next morning I went again to ſee the cows, examined them anew, and found on one of them four red ſpots already tumid and raiſed into puſtules; three of theſe were ſpread over the nipples, and the fourth lay in the middle of the dugs. The other cow had ſix puſtules; two on the nipples, and the reſt ſcattered above them. Theſe were larger than thoſe of the firſt cow, and around them appeared a ſlight red circle. Apparently theſe puſtules occaſioned much pain to the cows; for, on my approaching to examine them more minutely, they would ſcarcely permit me to touch them for one moment. Although the puſtules were already large and prominent, they did not yet appear to me ſufficiently mature to yield the matter I wanted. As the cows were that day to go forward on their way to Milan, I found myſelf under the neceſſity of following them to their firſt halting-place, in order to examine them again next day. I walked out at an early hour to the meadow where they were at paſture. I examined the puſtules, which appeared to me to be now arrived at maturity. They were lucid, and of a pale red colour, with a brown ſpot in the middle more depreſſed; and I thought this a favourable moment to collect the matter, which, through the aſſiſtance of the herdſmen, I was eaſily enabled to do by repeatedly ſoaking a thread in it. Although I ſaw no reaſon to doubt that this was the true cow-pox, yet, this being the firſt time I had ever ſeen it, I began to ſuſpect that the puſtules might be of that kind which Jenner calls the *ſpurious* cow-pox: I determined, therefore, to decide the matter by experiment. A conſiderable number of experiments, all uniform in their ſymptoms and progreſs, and always conſtant in their reſults, put the matter beyond doubt, and gave me full conviction that this was the true cow-pox. Such and ſo many are the obſtacles to be overcome on the introduction of any innovation, however ſalutary, that I for ſome time deſpaired of being able to induce any one to ſubmit to inoculation with the matter I had collected. In fine, after many fruitleſs perſuaſions, I ſucceeded in my deſign: the

ſucceſs

ſucceſs which attended the firſt inoculations encouraged others to ſubmit to the ſame proceſs."

Dr. Sacco then proceeds to detail three hundred caſes, in which he applied the pus he had obtained in the manner deſcribed above. Theſe caſes were attended with various circumſtances; but the inoculation ſucceeded to produce the cow-pox in all of them; and in a conſiderable number the inoculation for the ſmall-pox was afterwards applied, but without any effect.

ELECTRICITY AND LIGHT.

The following hint for electric experiments is communicated by a perſon who has not a room fitted up properly for making ſuch nice experiments himſelf:

The *electric matter* being ſuppoſed by ſome philoſophers to be the ſame as the *ether* mentioned by Newton, which he imagined conſtantly to reſide near the ſurface of all bodies, I wiſh to have the following experiments made by ſome of your readers, and the reſults communicated to the public by your Magazine.

Let ſmall wire bars be electrified *poſitively* and *negatively*, as it is called, and placed in a convenient ſituation, ſo that rays of light may paſs very near them, and obſerve whether any difference is produced either in the *reflection* or *inflection* of the rays when the bars are electrified; and what that difference, if any, is. Theſe experiments may perhaps give ſome inſight into the two ſtates of electricity, and inform us whether the poſitive is a *condenſed* ſtate, and the negative a *rarefied* ſtate, of the electric matter.

March 15, 1802.

ASTRONOMY.

The aſtrologers alarmed Europe in 1186 by announcing a conjunction of all the planets, which was to occaſion extraordinary ravage. I have ſpoken of this in the preface to my Aſtronomy: but, being deſirous to know whether this rare and ſingular phænomenon really took place that year, C. Flauguergues, aſſociate of the Inſtitute, a zealous aſtronomer, undertook to make the neceſſary calculations, and found, indeed, that on the 15th of September 1186 all the planets were comprehended between 6 ſigns, and 6 ſigns 10 degrees of longitude. This is not exactly a conjunction, but many thouſands of years perhaps muſt elapſe before there will be ſuch an approximation towards a conjunction.

——— De Lalande.

The ninth planet, diſcovered by Piazzi, affords occupation, and

and with great reaſon, to aſtronomers. C. Burckhardt has calculated the derangements which Jupiter may occaſion to it, and has thence deduced a new determination of the orbit. The mean diſtance, 2·7677; eccentricity, 0·0791; inclination, 0° 37′ 4″; epoch 1801, 2ˢ 17° 19′ 2″; aphelion, 10ˢ 26° 8′ 42″; the node, 2ˢ 21° 5′ 46″; revolution, 1681·51 days.

On the 8th Ventoſe (Feb. 27), at 13ʰ 59′ 15″ mean time, Lefrançois and Burckhardt determined exactly its right aſcenſion, 186° 58′ 44″, and its declination 15° 15′ 55″.

As we have been nearly a month without being able to obſerve it, I have ſent to all the French aſtronomers who reſide in the ſouth, the poſitions neceſſary for finding it. It appears now as a ſtar of the ſeventh magnitude.

DE LALANDE.

PROFESSOR VOLTA.

In a letter which we have received from profeſſor Pictet, of Geneva, we are informed that, beſides the golden medal voted to profeſſor Volta by the National Inſtitute, the French government has rewarded him for his diſcovery reſpecting galvaniſm with a preſent of 6000 francs. A few days after his arrival at Lyons, he received the following letter from the miniſter of the interior:

"The French government, citizen profeſſor, has granted to you a preſent of 6000 francs (250l. ſterling). It has thought this mark of eſteem due to the illuſtrious philoſopher who, after enriching ſcience with uſeful truths for twenty-five years, has come to depoſit in the Inſtitute the ſecret of nature and of the effects of galvaniſm. I am happy in being the organ of government to a man whom I eſteem, and for whom I have long entertained an affection. I beg you to accept the friendſhip of the conſuls. I ſalute you cordially.

"(Signed) CHAPTAL."

ACOUSTICS.

A commiſſion appointed by the French miniſter of the interior, and conſiſting of C. Lacepede, Prony, Charles, Goſſu, and Martini, have given in a report reſpecting an invention, by C. Montu, of an inſtrument which the commiſſioners ſay may be called a *ſono-metre*. Two of theſe inſtruments were ſubjected to their examination, one of them ſimple, and the other compound, in all its parts. They were both examined with great minuteneſs, and ſeemed to be the production of great ſkill and inventive genius. The name given to this inſtrument by the commiſſioners ſufficiently indicates the object of it: *ſono-metre*, a meaſurer of ſound. The commiſſion, both

both for the benefit of muſic and of the arts in general, have judged it proper that the French government ſhould purchaſe it at the price of 12,000 francs (500l. ſterling).

THE FINE ARTS.

The firſt number of Tiſchbein's Graphic Illuſtrations of Homer, a work long expected, has now appeared. This celebrated engraver, no leſs diſtinguiſhed by his talents as an artiſt than by his claſſical taſte, having become an early admirer of the poetical beauties of the Iliad and Odyſſey, reſolved to examine all thoſe remains of antiquity which have any relation to the poems of Homer, and, in order to give to his countrymen who had no opportunity of ſeeing thoſe maſter-pieces of art themſelves, at leaſt ſome idea of them, to make a collection of accurate drawings from the originals, and then to engrave and publiſh them. A reſidence of ten years in Italy, to which he was invited by his Neapolitan majeſty to be director of the academy of painting at Naples, and the acquaintance which he there formed with thoſe great patrons and promoters of the arts, Italinſky and Sir William Hamilton, procured him a connection with the firſt artiſts of Europe; by which means acceſs was opened for him to objects in different countries, and he was thus enabled to carry his long-projected plan into execution, notwithſtanding the multiplied repreſentations which were made to him reſpecting the difficulties to be encountered in ſuch an undertaking. Tiſchbein began his work, on which he inceſſantly laboured for ſeveral years, with uncommon zeal, patience, and aſſiduity, and at a very conſiderable expenſe. In this manner he formed a valuable ſeries of drawings, relating to, and illuſtrative of, the poems of Homer; and they no doubt would have been long ago publiſhed, had he not been interrupted in his peaceful occupation by the tumult of arms, and obliged, in conſequence of the taking of Naples by the French, to abandon Italy. By making great ſacrifices, both in regard to labour and money, he carried off in ſafety his valuable treaſure, among which was one of the firſt maſter-pieces of Raphael, a painting of St. John; together with his drawings for Homer, and ſuch of the plates as were already engraved. In the courſe of his paſſage by ſea, from Naples to Leghorn, a paſſage which carried him paſt ſeveral of thoſe iſlands celebrated in fabulous hiſtory, he experienced a violent ſtorm in the very place where the drawings of Leonardo da Vinci, and thoſe of Michael Angelo which he had executed for Dante, were formerly loſt; and the ſhip, being unable to weather the ſtorm, was driven on the

ſhores

shores of Corsica, and wrecked. Tischbein's good fortune, however, still attended him, and the drawings were saved. Retarded by such impediments thrown in his way by the waves of the sea, or the arms of the enemy, he was seven long months in reaching Cassel, where he left his valuable collection under the care of his brother, and repaired to Göttingen, to make arrangements for the publication of his work, under the inspection and with the assistance of the celebrated professor Heyne, with whom he had already corresponded, and invited to take a share in this important undertaking.

The German text of this work is from the pen of Heyne; and Villers, author of a Commentary on the Philosophy of Kant, gives a French translation of it. Each number contains six plates, and the whole when complete will form two volumes folio; one for the Iliad, and the other for the Odyssey. The numbers are to be published in such a manner, that one relating to the Iliad and another to the Odyssey will appear alternately. The first number contains a superb head of Homer, after the Farnesian bust; Homer instructed by the Muses, after a cameo; the apotheosis of Homer, from a silver cup; the rape of Helen, from a sarcophagus; the heads of the seven principal heroes, from seven antique busts; and the body of Antilochus placed on a car by Nestor, from a bas-relief in white marble.

MEASURING A DEGREE OF THE MERIDIAN IN LAPLAND.

The following extract of a letter on this subject from M. Melanderhielm, perpetual secretary of the Academy of Sciences of Stockholm, to C. Delambre, member of the French National Institute, dated October 9th and December 22d, will show what progress has been made in this operation.

"The three packages arrived safe three days ago. I have received the circle, the double metre with the toise, and the books. The whole were in good condition, and had sustained no damage by the way.

"Since these articles arrived, Messrs. Svanberg, Ofverbom and myself have been employed in examining all the parts of the circle, and comparing it with the complete description which you had the kindness to communicate to me. We have thus obtained a perfect knowledge of the mechanism of the instrument, the use of all its parts, and the method of making observations. As the prospect from the house where I reside is confined, I shall cause the circle to be

transported to the observatory of the academy, where I hope we shall be able to make all the observations necessary to prove it, and to acquire the habit of using it before it is conveyed to Tornea.

"The double metre, the toise, and the volumes, which the Institute presented to our academy, were also in good condition, and I beg you will present most respectful thanks to the Institute both on the part of the Academy of Sciences and on my own.

"In regard to the measurement in Lapland, I shall give you an account of every thing hitherto done. Messrs. Svanberg and Ofverbom had no need of assistants during the first journey which they made this year to examine the country; but at present, when the object is to measure the base and the angles, they will have occasion for two able and expert ones.

"In this respect I have found every thing I could desire. The first is M. Holmquist, mathematical assistant at Upsal, where he has laboured for several years in the observatory; the other is M. Palnder, teacher of mathematics in the university of Abo, in Finland. They are both well acquainted with the mathematics and astronomy, and animated with the strongest zeal for the success of the expedition.

"Messrs. Svanberg and Ofverbom returned hither in the beginning of October, their first journey having been attended with complete success. The only thing wanting, in my opinion, is, that, notwithstanding the strictest search, they were not able to find the northern point of the base of 1736. The uncertainty in regard to this point is about two toises. I consider this difference as a great defect, as I wished, above all things, to have exactly the same base, to render our verification more certain and conclusive. They have found all the other stations and points of the measurement of the above period. What they have done besides may be reduced to the following articles: they have erected all the signals necessary for continuing the arc, which will extend to lat. 67° 08′ 36″, almost 30′ beyond that of 1736. They have also given a peculiar construction to the signals, that they may be exactly sure of the point observed. I have sent you the figure and description of these signals. They have caused two new observatories to be built at the extremities of the arc, with huts to serve as lodging-places during the time of observation. But all their researches to find ground proper for a second base have been fruitless. We must be contented with measuring twice the same base as in 1736.

"In

"In regard to the degree of longitude, they have found four proper ſtations on each ſide of the baſe, but it has not been poſſible to extend it further. We ſhall therefore have but a degree at moſt, which in this latitude makes no more than about five or ſix French leagues.

"M. Ofverbom has made fruitleſs attempts to find the intermediate ſtations which divided into two parts ſome triangles a little too oblique, and which are exactly the ſame as thoſe pointed out in your letter. I ſuppoſe that the aſtronomers of 1736 did every thing in their power to have their triangles as convenient as poſſible. Like M. Ofverbom, they muſt have found inſurmountable obſtacles in the mountains and foreſts.

"If no unexpected incident retards their journey, Meſſrs. Svanberg and Ofverbom expect to ſet out for Lapland towards the middle of January next. By theſe means they may employ the months of February, March, April, and even part of May, in meaſuring the baſe on the river Tornea, ſince the ice ſeldom thaws in that country before the end of May."

From the concluſion of this letter it is to be preſumed that the Swediſh aſtronomers are now on their way to Lapland. To form a juſt idea of the zeal and courage which their enterpriſe requires, nothing is neceſſary but to read the work publiſhed in 1738 by Maupertuis under the title of *Figure de la Terre determinée par les Obſervations faites au Circle Polaire*; and particularly page 51, where an account is given of the meaſurement of the baſe, begun on the 21ſt of December 1736, and finiſhed the 27th of the ſame month. It is here ſeen that Meſſrs. Svanberg and Ofverbom propoſe to devote a much more conſiderable time to this operation, in order that no doubt may remain reſpecting the correctneſs of it. The angles which they mean to obſerve at the two extremities of their baſe, between ſignals placed in the very ſame points as thoſe of 1736, will perhaps enable them to find more exactly the northern term of the old baſe. At all events, what ought to leſſen the regret of M. Melanderhielm, as well as ours, is, that the triangle on this baſe was ſituated in the manner moſt favourable for making the diſtance between Avaſka and the ſouthern term independent of the ſmall errors unavoidable in the meaſurement of angles. The caſe is nearly the ſame with the ſecond triangle; ſo that the diſtance between Avaſka and Cuituper may, like that of Avaſka from the ſouthern term, give reſults as certain and as concluſive as the direct compariſon of the baſes of 1736 and 1801.

ANTI-

ANTIQUITIES.

The conqueſt of Egypt has enriched this country with a number of antient and rare monuments; ſome of them very entire, and of the higheſt and moſt undoubted antiquity. We ſome time ago announced * that the French, in digging up the earth at Fort Elleve, near the Bogar of Roſetta, had diſcovered a black granite containing inſcriptions in Greek, in the vulgar language of the time in which it was executed, and in hieroglyphical characters; and which inſcriptions, on an examination of the Greek one, appeared all to contain one decree of the Egyptian Prieſts in honour of Ptolemy Epiphanes. We are happy to announce that colonel Turner lately brought this valuable monument ſafe to England in his majeſty's ſhip Egyptienne. It may be conſidered as a treaſure to the learned of Europe; for, though the French had brought home impreſſions taken from it by different proceſſes, it is not poſſible that with theſe ſo much could be effected towards a complete tranſlation as by a view of the ſtone itſelf. The decree being the ſame in all the three characters, it is not an unreaſonable hope that the Greek copy will lead to a thorough underſtanding of the Coptic, and both to ſome knowledge of the hieroglyphical manner of writing. Should this be accompliſhed, what a field for reſearch is opened! Upper Egypt preſents to the aſtoniſhed traveller the ſuperb remains of immenſe palaces and temples filled with theſe characters; even the rocks in ſome places are covered with them, and they are found throughout the country from the mouths of the Nile to the borders of Ethiopia. Let us ſuppoſe for a moment we could decypher and explain them, what an intereſting volume would Egypt unfold to the hiſtorian, the antiquarian, and philoſopher! We ſhould become acquainted with the hiſtory of the firſt ages, now involved in impenetrable darkneſs; we ſhould view the ſacred writings and theſe venerable monuments explaining away the myſtical deſcriptions and expreſſions of each other, and ſee the laboured hypotheſis of many a learned man giving way on all ſides and tumbling into ruin.

CHEMISTRY.

By ſome recent experiments of C. Thenard it appears, that what has been for ſome time conſidered as a peculiar acid, under the name of the *zoonic*, is nothing but a peculiar combination of acetous acid with animal matter.

Some of the foreign chemiſts have announced that the ſuppoſed new metal, made known ſome time ago by Klaproth under the name of *tellurium*, turns out to be only *regulus of antimony*.

* Philoſophical Magazine, vol. viii. p. 94 and vol. ix. p. 141.

XXXVI. *On the Fusion of Malleable Iron with various Kinds of Glass; being a Continuation of the Examination of* C. CLOUET'S *Process for making Cast Steel. By* DAVID MUSHET, *Esq. of the Calder Iron Works**.

C. CLOUET, in his results of experiments upon iron and steel, states, that a particular modification of iron is formed by fusing malleable iron with glass; and infers, that this change is effected by the combination of a small portion of the glass with the metal.

In my last communication I had occasion to remark, that the mixtures there fused in contact with malleable iron entered into fusion a considerable time before the iron. It appeared, therefore, sufficiently obvious that a perfect glass was formed before any disposition to fusion was indicated on the part of the iron. Reasoning in this manner, I could distinguish no difference of situation betwixt iron exposed in contact with a glass formed by an union of clay and lime, and betwixt the same substance exposed to fusion in contact with window or bottle glass.

If an alteration of quality is effected, and this as being the consequence of an affinity exerted upon the glass by means of the iron, then it appeared probable that the affinity would exert itself in both cases, and be productive of similar effects. In short, since the carbonate of lime had failed in forming steel, it was conceived that the results of the same iron fused in contact with glass, would be analogous to those obtained with a mixture of iron, lime, and clay.

The following experiments are selected from a great variety performed to ascertain this point.

Exp. I. Fragments of the same Swedish iron formerly used - - - - Grains. 1312

Bottle glass - 1312 grains.

From this mixture was obtained a very perfect fusion, accompanied with a dense, smooth-skinned button of iron, which weighed - - - 1292

Lost in fusion 20

equal to $\frac{1}{65\frac{1}{2}}$ part of the original weight of the iron. The fracture of this button possessed a small regular grain of a light blue colour, more resembling the original fracture of

* Communicated by the Author.

the iron than any formerly obtained. One-half of the piece drew eaſily into ſhape, and formed a ſolid handſome bar. This was ſubjected to various teſts, in all of which it exactly reſembled the products obtained with the clay and carbonate formerly deſcribed.

The glaſs in this experiment, when compared with a maſs of the ſame fuſed *per ſe*, was of a darker green colour, poſſeſſed of an additional ſhade of amber.

Exp. II. The ſame experiment, attended with ſimilar reſults, was performed with equal portions of matter, and a loſs of $\frac{1}{85}$th part ſuſtained. Grains.

Exp. III. Fragments of the ſame Swediſh iron - 1103
Bottle glaſs - 551½ grains.
The reſult in 20 minutes was a finely formed button, weighing - - - - 1090

Loſs, equal to $\frac{1}{85}$th part, 13

This experiment was performed to aſcertain if the quality of iron was affected by a reduction of the vitrid matter. Nothing materially different as to quality could be inferred. The fracture of the button approached to a regular cryſtallization, in which ſome very perfect cubes of $\frac{1}{10}$th inch upon the ſide were formed. Grains.

Exp. IV. Fragments of Swediſh iron - 875
Bottle glaſs - 1750 grains.
There reſulted from this mixture a very perfect fuſion, accompanied by a very beautifully cryſtallized button of metal, weighing - - - 860

Loſt in fuſion 15

equal to $\frac{1}{58\frac{3}{10}}$th part of the original weight of the iron. The fracture of this product preſented groups of large flat granulated cryſtals reſembling in point of colour No. 3. One-half of the button forged with every appearance of ſoftneſs and malleability, and formed a ſolid bar. In ſubſequent teſts, no ſhade of diſtinction could be found betwixt this and the quality of the three former reſults.

It would therefore appear concluſive, that Swediſh iron, by fuſion with bottle glaſs, forms a ſpecies of *caſt malleable iron* exactly ſimilar in its appearance and properties to that formerly obtained when carbonate of lime and argil were uſed. It is further inferable that the reſulting products are not materially affected by various proportions of glaſs, ſeeing that an equal portion, a half, and even a double portion, by weight, to the iron, were productive of no perceptible alteration.

tion. Neither does it appear that the quantity of deficient metal is in the ratio of the quantity of glaſs employed.

		Grains.
Equal portions of glaſs and iron.	Loſs in metal	65·5
Ditto - ditto	Ditto - -	85
Glaſs half the weight of iron.	Ditto -	85
Glaſs double the weight of iron.	Ditto - -	58·3

Average loſs equal to $\frac{1}{73\frac{45}{100}}$

Grains.

Exp. V. Swediſh iron - - - 900

Flint glaſs pounded - 900 grains.

This mixture was expoſed to a pretty high heat, which effected a very perfect reduction. The glaſs was ſmooth and denſe upon the ſurface. The button of iron - 855

Loſt in fuſion 45

equal to $\frac{1}{20}$th part of the original weight of the iron. One-half of this button drew pleaſantly into ſhape, and formed a ſound bar, which, when ſubjected to various trials, indicated none of that want of tenacity deſcribed by Clouet, but in every reſpect reſembled the qualities of metal deſcribed in my two laſt communications. The glaſs obtained in this experiment was of a greeniſh blue lead colour, a little tranſparent when broken into thin fragments. Beneath the button of iron I found a ſmall maſs of revived lead which weighed 135 grains, equal to $\frac{1}{67\frac{5}{10}}$th part of the weight of the flint glaſs.

Grains.

Exp. VI. Swediſh iron - - 3500

Flint glaſs $\frac{1}{2}$, or 1750 grains.

The mixture was expoſed for an hour to a violent heat. A fuſion was obtained, and the button of metal weighed - - - - 3380

Loſt in fuſion 120

equal to $\frac{1}{29\frac{1}{10}}$th part of the original weight of the iron. This product was cut into two pieces, one of which was drawn into a ſolid bar, under a pretty bright red heat. It afterwards ſtood hammering, with caution, at a welding heat, but was ſo completely red-ſhort that it would not turn at any ſhade above a faint red. The fracture of this bar, when cold, was partly granulated and partly ſilky fibrous, of a very light colour. The glaſs approached more to a perfect green than that of No. V; ſtill, however, faintly tinged with a ſhade of

lead. Under the iron was found of revived lead 378 grains, equal to $\frac{1}{4\frac{6}{10}}$th part the weight of the flint glaſs.

	Grains.
Exp. VII. Swediſh iron - -	882
Crown window glaſs - 882 grains.	
From this mixture reſulted a very complete fuſion. The metallic button was minutely but regularly cryſtallized upon its upper ſurface. I found it to weigh	868
Loſt in fuſion	14

equal to $\frac{1}{53\frac{4}{10}}$th part the original weight of the iron employed. The fracture of this metal was ſilvery white, parting in laminæ, deſtitute both of fibre and grain. This appearance was quite new, and more like zinc than any fracture of iron I had ever before ſeen. Suffice it to ſay, that when ſubjected to trial its quality was preciſely the ſame as found in the former reſults. The glaſs was light green, reſembling that of No. III.

	Grains.
Exp. VIII. Swediſh iron - -	560
Window glaſs double in weight, or 1120 grains. This mixture was reduced to a perfect fuſion, and a cryſtallized button of metal obtained marked with various priſmatic ſhades. It was found to weigh	553
Loſt in fuſion	7

equal to $\frac{1}{80}$th part the weight of the iron. The fracture of this button was granulated, and in point of appearance and luſtre differed conſiderably from No. VII. It drew, however, eaſily into ſhape, and felt ſoft and very ductile under the hammer. I ventured to double-weld the end of the bar; which it ſtood pretty well, a few edge cracks excepted. The glaſs was of a blueiſh green caſt, abounding with waving lines of a faint garnet colour. Thin fragments, however, diſplayed a graſſy green, conſiderably tranſparent.

It ſeems deciſive, from theſe experiments, that the quality of the metal obtained by the fuſion of bottle glaſs, flint glaſs, and crown glaſs, with the ſame quality of iron, was productive of exactly ſimilar reſults to thoſe deſcribed in former papers, where Swediſh iron was uſed with various proportions of argil and carbonate of lime. None of theſe products exhibited any properties of ſteel, but all of them conſiderably altered. The cauſe of alteration ſtill remains to be developed. If glaſs has an affinity to iron, this muſt have been exerted in all theſe fuſions alike, whether the glaſs was introduced

introduced already formed into the crucible, or afterwards produced by the fuſion of one or more earths. In all, ſimilar deficiencies of weight have been experienced. This accords not with a combination of glaſs, which ought to have added weight to the reſults.

Having thus minutely ſtated the reſults of various experiments here detailed, and claſſed their qualities with that of others obtained by different mixtures, and communicated in two former papers, I ſhall next exhibit ſome experiments with Britiſh cold-ſhort iron, wherein ſome reſults approach very nearly to that deſcription of *caſt malleable iron* deſcribed by Clouet as combined with glaſs, and incapable of diſtending under the hammer.

	Grains.
Exp. IX. Britiſh cold-ſhort iron - -	1750
Bottle glaſs - 1750 grains.	
There reſulted from the fuſion of this mixture an elegantly cryſtallized button of iron, weighing -	1663
Loſt in fuſion, equal to $\frac{1}{19\frac{1}{10}}$th part,	87

This button, without being broken, was put under the hammer, but would not draw into ſhape at the loweſt red heat. Again this was repeated as follows:

	Grains.
Exp. X. Britiſh cold-ſhort iron - -	3500
Bottle glaſs - 3500 grains.	
A very perfect fuſion alſo reſulted from this mixture. The button was leſs perfectly cryſtallized, and weighed	3320
Loſt in fuſion, equal to $\frac{1}{19\frac{4}{10}}$th part,	180

This maſs was alſo found to be utterly incapable of diſtenſion under the hammer at any degree of heat.

	Grains.
Exp. XI. Britiſh cold-ſhort iron - -	875
Bottle glaſs - 875 grains.	
Another elegantly cryſtallized button was here obtained, which weighed - -	831
Loſt in fuſion, equal to $\frac{1}{19\frac{8}{10}}$th part,	44

This button was as incapable of hammering and diſtenſion as the former. The accuracy of theſe reſults may be inferred from the reſpective deficiencies of the weight. The quality of the whole ſeems to be nearly that deſcribed by Clouet.

	Grains.
Exp. XII. Britiſh cold-ſhort iron - -	875
Bottle glaſs - 437½ grains.	

Grains.

Brought over 875

A very fine cryſtallized button was obtained from this mixture, which weighed - - - 808

Loſt in fuſion, equal to $\frac{1}{13\frac{1}{2}}$th part the weight of iron, 67

The fracture of this button was completely deſtitute of grain, laminated, ſmooth, and of a ſilvery white colour. With much caution one-half of it was drawn into a ſmall bar; but in endeavouring to form it into a point, it became looſe, and incapable of further diſtenſion. It felt conſiderably harder under the hammer than the products from Swediſh iron. This reſult, upon the whole, was more ductile than any of the former, and the quantity of glaſs uſed leſs.

Grains.

Exp. XIII. Britiſh cold-ſhort iron - - 875

This was expoſed *per ſe* to a very high heat, and a perfect button of metal was obtained covered by a thin film of brown glaſs. The iron weighed - 805

Loſt in fuſion, equal to $\frac{1}{11\frac{1}{2}}$th part the weight of iron, 70

This button poſſeſſed a brilliant, flat, granulated fracture. One-half of it was drawn into a very ſolid bar poſſeſſing ductility and ſoftneſs in an eminent degree. The quality of this iron was ſo completely cold-ſhort before fuſion, that a ſtout bar of it was eaſily broken by a blow from a hand hammer. It now poſſeſſed the oppoſite extreme of red-ſhort, and was ſo pliant when cold, that a bar nearly $\frac{1}{5}$ths ſquare folded cloſe by the compreſſion of the vice, and afterwards opened fairly, and was bent backwards and forwards ſeven times before it cracked.

Grains.

Exp. XIV. Britiſh cold-ſhort iron - - 500

Bottle glaſs - 1000 grains.

A very beautifully cryſtallized button was obtained by the fuſion of this mixture, which weighed - 473

Loſt in fuſion, equal to $\frac{1}{18\frac{1}{2}}$th part of the weight of iron, 27

The fracture of this iron diſplayed a double row of very perfect cubes inſerted in each other. The colour was a ſilky light blue, not ſo dazzling as in the former experiment. One-half of this button was ſmooth filed upon the fracture, and forged at a low heat, to endeavour, if poſſible, to obtain a ſolid bar. It however cracked conſiderably, and became looſe and ſhaly. The hardened fracture poſſeſſed a flat, crowded, indiſtinct grain, of a dazzling ſilvery colour; the ſurface turned up a gray ſpotted ſhale, reſembling that of caſt ſteel.

It

It would appear from theſe experiments with bottle glaſs and Britiſh cold-ſhort iron, that a quality of caſt malleable iron is obtained, different, in general, from the products obtained with Swediſh iron; that the leaſt ductile iron is formed when equal portions of glaſs and iron are uſed; and, that the moſt ductile ſtate reſults from fuſing the iron *per ſe*.

It cannot, however, be inferred from this, that the general want of ductility is owing to a combination of glaſs with the iron, ſeeing that when Swediſh iron was employed under ſimilar circumſtances no want of ductility was perceptible; I would rather infer, that the difference betwixt the fuſed reſults was characteriſtic both of the ſpecies of the iron and the nature of the manufacture.

The following experiments were performed with flint and window glaſs, to prove whether any part of the difference, in point of quality, aroſe from the nature of the glaſs uſed.

	Grains.
Exp. XV. Britiſh cold-ſhort iron -	875
Flint glaſs - 875 grains.	
The metallic button reſulting from this fuſion was denſe and very ſmooth ſkinned, entirely free from traces of cryſtallization. It weighed - -	792
Loſt in fuſion, equal to $\frac{1}{10\frac{1}{4}}$th part,	83

The fracture of the button was compoſed of large facets, bright, and inclining to a cubical ſtructure. One-half of the button drew into a looſe ſhaly bar, conſiderably cracked upon the edges. In point of quality it reſembled products XII and XIV. A button of ſoft lead was found beneath the iron weighing 133 grains, equal to $\frac{1}{6\frac{1}{2}}$ part the weight of the glaſs.

	Grains.
Exp. XVI. Britiſh cold-ſhort iron - -	3500
Flint glaſs $\frac{1}{2}$, or 1750 grains.	
A perfect fuſion of this maſs was obtained in an hour. The metallic maſs weighed - -	3317
Loſt in fuſion, equal to $\frac{1}{13\frac{6}{10}}$th part,	183

The fracture of this button was a mixture of granulated cryſtals, and bright laminated facets, ſomewhat inclined on edge. The half of the button drew into ſhape, and formed a bar ſomewhat looſe and cracked. Upon the whole, the difference betwixt XII, XIV, XV, and this, was ſcarcely perceptible.

ceptible. The quantity of revived lead amounted to $\frac{1}{6\frac{7}{10}}$th part the original weight of the flint glaſs*.

	Grains.
Exp. XVII. Britiſh cold-ſhort iron - -	875
Window glaſs - 875 grains.	
From this mixture, in half an hour, was obtained a fine fuſion accompanied by an elegantly cryſtallized metallic button weighing - - -	836
Loſt in fuſion, equal to $\frac{1}{22\frac{4}{10}}$th part,	39

The fracture of this button was compoſed of irregular cubes inſerted in each other. A bar forged from the half of this button was looſe, and cracked in the edges. Its quality was much akin to Exp. XVI.

	Grains.
Exp. XVIII. Britiſh cold-ſhort iron - -	500
Window glaſs - 1500 grains.	
The button obtained from a fuſion of the mixture weighed - - - -	453
Loſt in fuſion, equal to $\frac{1}{10\frac{6}{10}}$th part,	47

The quality of the metal now obtained was equally incapable of being forged as that in Exp. IX and X.

From the nature of the reſults of theſe experiments it cannot be ſatisfactorily concluded that even the fuſion of Britiſh cold-ſhort iron with glaſſes is uniformly productive of the ſame quality of metal. But it may be fairly deduced that it invariably is productive of a caſt malleable iron, harder and much leſs ductile in every particular than the reſults from Swediſh iron ſimilarly treated.

The moſt probable cauſe of this irregularity of quality in the cold-ſhort iron I conceive to be cryſtallization. In the bar, this quality of iron always preſents cubes more or leſs diſtinct. When fuſed, and allowed to cool under the circumſtances neceſſary to produce this effect, a large perfectly cryſtallized fracture is the conſequence. This does not invariably happen; but in two experiments out of three, I have found it to be the caſe. In a button of 10 to 1200 grains, theſe cubes are found meaſuring a full quarter of an inch upon the facet; and ſuch ſometimes has been the degree of diviſion of the fluid, and a conſequent perfect cryſtalliza-

* In this paper I have repeatedly ſtated the fact of oxide of lead being revived when no carbonaceous matter (that in the iron excepted) was preſent. I ſhall paſs over any inference to be made from this circumſtance, until a ſubject more immediately connected with this curious fact is brought forward in its proper place.

tion,

tion, that they appear to have to each other a very ſlight bond of union. It appears therefore to me obvious, that when a maſs ſuch as is now deſcribed is put under the hammer, the parts will tend to repel each other, unleſs that preciſe degree of heat could be aſcertained which would coaleſce the cryſtals without either parting or diſſipating them.

In theſe experiments four various fractures of caſt malleable iron have been obtained: regularly granulated, more or leſs, on edge: laminated, parting ſometimes entire, at other times in large flat facets: cubical, more or leſs perfect: mixed, compoſed of the former three.

The firſt of theſe, except in one inſtance, was peculiar to the Swediſh iron; the ſecond were common to both; the third peculiar to the cold-ſhort iron, one inſtance excepted; the fourth was common to both.

		Grains.
Exp. XIX. Britiſh cold-ſhort iron	- -	875
Kilkenny marble -	875 grains.	
Stourbridge clay (old) pot	875 grains.	
A beautiful ſmooth-ſkinned button was obtained from a fuſion of this mixture, which weighed	-	813
Loſt in fuſion, equal to $\frac{1}{14\frac{3}{10}}$th part the weight of iron,		62

This button ſplit in circular laminæ, and exhibited, in ſome reſpects, an entirely new appearance. It forged with more facility than any of the buttons obtained with glaſs, and was now uncommonly tough when cold.

		Grains.
Exp. XX. Britiſh cold-ſhort iron	- -	500
Kilkenny marble -	750 grains.	
Stourbridge clay (old) pot	750 grains.	
A very perfect fuſion was the reſult of this expoſure, accompanied by a very ſmooth button of metal, on which were impreſſed ſome faint traces of the uſual cryſtallization. It weighed	- - -	468
Loſt in fuſion,		32

equal to $\frac{1}{15\frac{6}{10}}$th part the original weight of iron. The fracture of this was poſſeſſed of a regularly granulated form, the cryſtals flat, and placed conſiderably on edge. It did not in the leaſt reſemble the former product, but in forging afforded no mark ſufficient to diſtinguiſh any difference betwixt their qualities.

In all theſe experiments we invariably find that the change upon the quality of the iron is great. Fuſion invariably communicates a great degree of red-ſhortneſs to all qualities; but with

with cold-ſhort iron, the extent of the change is by far the moſt conſiderable. Its loſs by weight is alſo much beyond that ſuſtained by Swediſh iron.

I meant to have inſerted a few experiments with iron uncommonly red-ſhort; but the number already adduced will, I am afraid, have the appearance of prolixity. Suffice it to ſay, that I have obtained data ſufficient to be able to proſecute, with ſome hope of ſucceſs, an inveſtigation of the cauſes of red- and cold-ſhort properties in malleable iron. I ſhall conclude this ſubject with the following remarks:—Iron fuſed with window glaſs always has its upper ſurface impreſſed with a beautiful cryſtallization, often coloured. This circumſtance never takes place when flint glaſs is uſed. When bottle glaſs is uſed, the cryſtallization is not ſo frequent, and ſeldom ſo perfect, as with window glaſs. Cryſtallization is ſtill leſs frequent, and leſs perfect, when a mixture of clay and lime is employed. When carbonate alone is fuſed with iron, the ſame want of cryſtallization takes place as when flint glaſs is uſed.

The fuſion of malleable iron is productive of the diſengagement of an elaſtic fluid, the eſcape of which, I am inclined to think, is one of the chief cauſes of the alteration of its quality. In moſt caſes, when the cover was ſtruck off while the matter was ſtill red-hot, a burſt of whitiſh blue flame iſſued from the mouth of the crucible. In others, where the fuſion had been of ſhort duration, or leſs perfect, the upper ſurface of the glaſs became covered with thin tranſparent bubbles, each of which appeared filled with a dull lambent blue vapour. Theſe were eaſily perforated with a pin point, and a ſlight exploſion was heard from each, accompanied by a momentary combuſtion. In burning, this ſubſtance reſembled hydrocarbonate *.

Experiment, which was omitted in the former communication, to prove that the carbonic acid was neutral in the fuſion of iron, or at leaſt was not productive of ſteel.

	Grains.
Italian raw marble - -	600
Steel oxide, from the tilt anvil block -	50

This mixture was reduced to a fine powder, and completely triturated. It was then introduced into a Stourbridge clay pot, and expoſed to a heat ſufficient to fuſe it. When cold,

* This fact may lead to a more ſimple explanation of the formation of inflammable air than that adopted by Dr. Prieſtley or Mr. Cruikſhank. Bar iron with a mixture of glaſs, without the addition of carbon or its acid in lime, are here productive of an elaſtic ſubſtance poſſeſſed of ſimilar properties as that obtained in their experiments.

the

the vitrid mass was minutely examined, but no metallic globule was visible. The mixture before fusion was magnetic, owing to the oxide. This property was now entirely lost.

The heat of this experiment was urged moderately, that time might be given for the exertion of any affinity, if such existed, betwixt the iron and the carbonic acid, or betwixt the oxide of iron and the carbonaceous part of the acid. No portion of metal being revived, I conceived this a most conclusive proof of the nondecomposition of the carbonic acid.

XXXVII. *History of Astronomy for the Year* 1801. *By* Jerome Lalande.

[Concluded from p. 121.]

M. BODE, of Berlin, has published the last part of his large Celestial Atlas in twenty sheets, which contains all the old constellations, with several new ones, and some thousands of stars, with which I furnished him; an immense labour, of which the astronomers had need. This beautiful work may be procured at the Collége de France.

On the 27th of September the Helvetic republic adopted the French measures. This is the first of the European states which has been sensible of the importance of this universal measure to the general good of civilized nations.

Guglielmini, of Bologna, has made three new experiments on the fall of bodies, to prove the rotation of the earth: he has found the same deviation from the south within a line, though it is not given by theory; but the deviation from the west he has found as it ought to be. Preparations are making for observations of the same kind at Hamburgh from the tower of St. Michael, at the height of 326 feet.

The observatory of Cadiz, during several years, has furnished us with a series of important observations; but for some time it has been neglected. General Mazzaredo has caused a new one to be built in the Isle-de-Leon, and he has attached to it four astronomers, officers in the navy—Rodrigo Armesto, Maximo-Lariva Aguero, Julian Canela, and Joseph Cuesta, who have resided there four years. For ten years past, a nautical almanac has been published in Spain. I hope navigation and astronomy will be benefited by it. The telescope 25 feet in length, made by Dr. Herschel for Spain, will be sent off in the month of January; and Dupont will go to Spain to mount it.

M. Travassos,

M. Travassos, secretary of the Academy of Lisbon, has sent me observations by M. Ciera, which have confirmed the longitude of that city; the Nautical Ephemerides published to 1803, and various works of the Portuguese Academy, of which we had no idea, and which the National Institute of France received with much interest. This negotiation was conducted by the chevalier d'Aranjo.

Astronomy was long in a languishing condition in the Batavian republic: M. Fokker has established, at his own expenses, an observatory at Middleburg; he has purchased instruments, and sent us several observations made between 1797 and 1801. M. Fokker, during the revolution of 1795, was member of the committee of public safety, and at that time obtained a tower in the abbey; but the revolution of the 12th of June 1796 interrupted his plans for the improvement of the observatory. He is now engaged in the finance department of Zealand; but his spare time is employed on astronomy, and he has sent me several interesting observations.

In Germany astronomy continues to be cultivated with great assiduity. Baron Von Zach's tour to Bremen and Lilienthal has produced new activity; and the society formed for the purpose of searching the heavens are still occupied with that object. He observes the moon with great diligence; and gives me reason to hope, that I shall see next summer a part of the German astronomers assemble in an astronomical congress at Gotha, as was the case in 1798. Amidst the horrors of war, the French astronomers signalized their zeal for astronomy. General Moreau, being at Cremsmunster, where there is a celebrated observatory, caused a bill to be posted up denouncing the punishment of death against every person who should be guilty there of any depredation; and neither the observatory nor the convent of the Benedictines sustained any injury. It is flattering to the French to have officers who distinguish themselves by a taste for the sciences. It will no longer be said that military men, in consequence of their situation, are ignorant and ferocious.

The Academy of Petersburgh has requested an observer, but Burg and Wurm have been retained by their sovereigns; and this beautiful observatory is still useless, notwithstanding the number of excellent instruments with which it is furnished.

C. Henry has had the satisfaction of erecting the large mural quadrant by Bird, and of making some observations with it.

The irregularity in the degrees of the earth hitherto measured,

sured, gave reason to suspect some error in that of Lapland measured in 1736. M. Melanderhielm, therefore, has obtained permission from the king of Sweden to undertake a new measurement. In the month of April Messrs. Ofverbom and Swamberg set out for Tornea, where they erected signals and built small observatories. When the ice on the river is thawed, they will measure a base with the rules sent them by the Institute. A multiplying circle, made at Paris by Lenoir, will serve them in the spring for measuring the angles; and next summer we shall have the solution of this old difficulty.

M. de Mendoza, a Spanish officer, has published two large collections of tables; one at Madrid, in 1800, entitled *Colleccion de Tablas*; and another at London, in the month of April 1801, which contains tables for the reduction of distances by the addition of five natural numbers: he has made a new use of the versed sines, which renders numerical operations shorter and easier. These tables consist of 407 pages quarto.

M. Garrard has published tables in thirteen pages only; but his method is neither shorter nor so accurate.

Mr. Vince, an able English astronomer, has published the second volume of a large treatise on astronomy.

The stereotype tables of logarithms, published by Firmin Didot in 1795, have been again corrected. M. Vega, who has caused to be printed in Germany the largest collection extant, has verified the French tables, and sent us several faults, which are going to be corrected: in all probability they will be the last, and we may depend in future on correct tables. This is a great benefit for calculators, who have sometimes lost whole days in revising calculations, which did not agree, in consequence of an erroneous figure.

But as the small manual tables are most frequently employed, I have caused them to be printed in stereotype: several persons have corrected them; and in three months I can give all calculators the most correct, most convenient, and most elegant edition that has ever appeared.

C. Verniquet has finished the engraving of his large plan of Paris in 72 sheets on a scale of half a line to the toise, which in correctness surpasses every thing of the kind.

A project was long ago formed and undertaken for making a lunar globe representing all the mountains and craters. Mr. Russel has accomplished this object in England: his lunar globe, mounted on an ingenious stand, expresses all the circumstances of the moon's libration, and shows that body as she ought to appear in the different positions of the

earth

earth and moon, as well as the variations of the equator and orbit.

M. Philippides, born at Mount Pelion in Thessaly, who attended the course of astronomy at the Collége de France in 1794, and who is now at Jassi with the hospodar of Moldavia, proposes to publish in Greek the Abridgement of my Astronomy: he has already published various works, for the purpose of endeavouring to propagate instruction in his country.

Three-fourths of the two last volumes of Montucla's History of the Mathematics are printed. This work will contain the history of astronomy, optics, and navigation; to which I have been obliged to make great additions in consequence of the too premature death of the learned author.

M. Von Murr, of Nuremberg, who has manuscripts of Regiomontanus, the first restorer of astronomy before 1500, has caused a page to be engraven, an exact fac-simile of the character of the manuscript: he offers to sell these manuscripts for 2400 francs; they would be a treasure to a large library.

The astronomical poems of Ricard, Lemiere, and Fontanes, had before shown how far a view of the heavens is capable of exciting poetical enthusiasm. C. Gudin has again proved it by a poem, which contains both the history of astronomy and a description of the heavens, and which displays as much correctness as elegance.

This year geography also has made considerable progress. Tranchot is constructing a map of the four united departments on the scale of a line to 100 toises: a survey is taking of the country between the Adige and the Adda, Piedmont, Swabia, and Switzerland; and the minister at war caused the details to be inserted in the Moniteur of August 14.

C. Henry, who has been invited to Munich to construct the map of Bavaria, informs me in a letter that the topographical part is in great forwardness; a base of 21,649 metres or 11,108 toises has been measured: it is the longest ever measured. The large triangles around the capital are already in part closed. There are some the sides of which will be from 15 to 20 leagues, and even more. He has already swept the horizon several times with his circle, and with astonishing precision. The last sweep was composed of six angles; the sum of which when reduced was not in excess, but 8-10ths of a second in 360 degrees; and yet the circle he used was not very good. To make up as much as possible for what may be wanting in regard to precision, he multiplies his observations: he never makes less than 15 conjugate observa-

tions, and he often carries the number to 20. The triangles which Cassini assumed in the neighbourhood of Munich are badly chosen, and the measurement of them is very incorrect. Without employing his triangles, Henry has already disposed a series of 14 triangles, the measure of which will give us that of an arc of the meridian of somewhat more than a degree: he hopes that it will still be possible to prolong this arc, which will pass at a little distance from Ingoldstadt, and which will ascertain the positions of a part of Germany. The travels of baron Von Zach and several of his co-operators have also supplied us with new information and new positions, which will improve the geography of Germany. Colonel le Cocq continues his map of Westphalia.

Baron Von Ende, member of the supreme council of appeal at Cette, has published a volume on the determination of several places in Lower Saxony: it is filled with observations and calculations.

The geography of distant countries has assumed also a new activity. Captain Baudin, whose voyage of discovery I before announced, left the Canaries on the 24th of November, and the Isle of France on the 22d of March. We have reason to hope that he has already made interesting discoveries in New Holland; the only country of the earth which is almost unknown to us, though it is 2000 leagues in circumference. Bernier, the astronomer who accompanies him on the expedition, a man of intelligence and courage, leaves nothing to be wished for on that head. In the month of June the French government granted passports to the English vessels, the Investigator, captain Flinders, on the point of proceeding on a voyage of discovery to the South Seas, and to the Lady Nelson, commanded by lieutenant Grant, who is to accompany the Investigator, in exploring the coasts of New Wales.

C. Deguignes jun., arrived from China, where he resided from 1784 to 1797, will, in all probability, when he publishes the journal of his voyage, give us some information respecting that beautiful part of the world.

Baron Humboldt, an enlightened and intrepid philosopher, has gone to South America, where he has travelled 1300 leagues in the deserts, with great labour and amidst terrible dangers, to make us acquainted with the geography and natural history of those countries which are still new to us.

M. Deferrer has sent me observations which give the position of Natchetz in Louisiana, and of Guaira in South America; for the former, lat. 31° 33′ 48″, difference of meridian 6^h 15′ 21″; and for the latter 10° 36′ 40″ N. and 4^h 37′ 11″.

C. Nouet

C. Nouet has sent us from Egypt an almanac calculated for that country, and several positions of cities even in Upper Egypt, notwithstanding the climate, the dangers, and inconceivable labour which such observations require. The value of the degree is 56,880 toises; the Egyptian stadium 711 feet; the Egyptian cubit 21·33 inches; the Greek stadium 487·543 feet, and the cubit 19·5017 inches. In a word, he has arrived himself, and brought us the continuation of his labours, accompanied with young Isaac Mechain, the son of one of our most celebrated astronomers, who was his companion and co-operator in Egypt. C. Fourrier has brought us drawings of the zodiacs of Upper Egypt, which attest the high antiquity of astronomy; and he proves that the formation of the constellations goes back 14,000 years, as Dupuis presumed.

C. Marquis, præfect of La Meurthe, has sent to the Board of Longitude observations and manuscripts of P. Barlet, a jesuit of Nancy, which contain interesting things.

I must here say a word of meteorology. C. Lamarc has published a meteorological journal, in which he gives a great many observations, and indicates the variations of the seasons which may be supposed to take place in the course of the year. The minister of the interior has established a meteorological correspondence to multiply observations; and Lamarc, who solicited this establishment, will make it advantageous to the science, which is still in its infancy.

C. Burckhardt, also, has written a long and curious work on meteorology. He has examined 15,000 observations of the barometer, that he may be able to calculate the influence of the winds; and he has found that the south wind gives for mean height 27 inches 11·3 lines, while the east gives 28 inches 1·9 line. He has found also that the height on the borders of the Mediterranean sea is 28 inches 2·2 lines, and on those of the ocean 28 inches 2·8 lines.

Well placed weather-cocks are very rare at Paris. There is none at the observatory, though I requested one on being appointed director; and I have thanked, in name of all observers, C. Bois, tinman, who having built a house on the Quai des Augustins, has erected there a lofty and very moveable weather-cock, with letters indicating the four cardinal points, which will be on a line with a meridian I have traced out on the quay. Astronomers, when they go to the Institute or the Board of Longitude, will have an opportunity of seeing conveniently the direction of the wind; and the same advantage will be enjoyed by the inhabitants of that vast quay, of the Louvre, and the surrounding houses, which had

had not a ſingle weather-cock in their view, but a great many conductors, which are not very intereſting.

On the 3d of November there was in the Baltic a terrible ſtorm, which deſtroyed ſome veſſels, and was felt even at Breſt. On the 7th there was a ſtorm in Provence, which produced 73 lines of water in 2 hours by a wind at S.S.E. It did very great damage at Marſeilles; ſeveral perſons periſhed in the neighbourhood, and the loſs amounts to ſome millions. C. Thulis has found ſome memorandums of the ſtorms of July 12, 1748; September 4, 1764; and September 15, 1772: but no perſon had any remembrance of a ſtorm like that of the preſent year. The plain of the Po was expoſed to an immenſe inundation.

The Claſs of the Phyſical and Mathematical Sciences on the 16th of April choſe three aſtronomers, who were preſented to the general aſſembly for the place of aſſociate, vacant by the death of C. St. Jacques, viz. C. Vidal, Sepmanville, and Bernard.

The firſt is an uncommon obſerver, who has alone made more obſervations of Mercury than all the aſtronomers of the world ſince 2000 years. The ſection of aſtronomy had preſented alſo C. Pictet of Geneva, Chabrol (de Riom), and Quenot, officers in the navy. I even made out a liſt of the aſtronomers known in France, which contained C. Henry, returned from Peterſburgh; Nouet and Beauchamp, returned from the Levant; C. Deratte and Poitevin, of Montpellier; Bernier and Biſſy, who embarked with captain Baudin; Chevalier, in the department of foreign affairs; Kramp, of Cologne; Duvaucel, at Evreux; Guerin, at Amboiſe; Mongin, at la Grand-Combe-des-Bois; Maingon and Lancelin, at Breſt; Jacotot, at Dijon; Planpain and Degrand, at Marſeilles. If we add to theſe the ſix aſtronomers aſſociated at the Inſtitute, it will be ſeen that this ſcience, the moſt unprofitable and the moſt neglected, ſtill furniſhes ſubjects in France. As ſoon as the happy event of peace exalted the hopes of literary men, I took advantage of it to ſolicit from all quarters, that aſtronomy might participate in the benefits of it.

The Academy of Peterſburgh has tranſmitted to me a ſmall preſent, which it has been accuſtomed for thirty years to ſend me for the good of aſtronomy; and the emperor of Ruſſia has approved the deſire of the Academy in that reſpect.

The king of Etruria has promiſed to patronize aſtronomy at Florence. The obſervatory is already provided with excellent inſtruments, and Fabroni has aſſured me that an obſerver will be placed in it: he has requeſted one of my pu-

pils; and this circumstance makes me regret that I have not a greater number.

General Jourdan gives me reason to hope that the observatory of Turin will be put in a proper state; and C. Vassalli, president of the academy, affords me hopes also.

The minister of the marine has given orders that new observations shall be made at Brest on the tides, according to my request, in order to complete my *Traité du Flux et du Reflux de la Mer*, which I wrote to confirm the excellent theory of Laplace in his *Méchanique Céleste*, and to ascertain what influence the wind has on the tides.

We requested the first consul to procure us from Spain two thousand pounds weight of platina, to construct a telescope of 36 feet; and we have reason to hope for it. Our telescope will, perhaps, surpass that of Herschel.

The observatory of Paris has acquired C. Agoustene. The minister of the interior, C. Chaptal, has agreed that the Board of Longitude may increase its expenses for this new assistant; and I have obtained C. Giroult, whose youth and assiduity give me new aid, and leave me no other regret than that of not being able to procure a greater number.

In my History of Astronomy for 1800, I mentioned the loss which astronomy had sustained on the 5th of November that year by the death of Ramsden: to him we have been indebted, during the course of twenty years, for the best and largest instruments, the most perfect telescopes, and the most ingenious ideas. Troughton, at present, is the most celebrated artist in England, and is preparing to indemnify us for this loss. He has already made excellent instruments; and C. Pictet, of Geneva, lately brought us some of them.

On the 10th of February we lost C. St. Jacques de Sylvabelle, director of the observatory of Marseilles, who distinguished himself by theoretical researches in 1753, as may be seen in the Philosophical Transactions, and then by useful observations: he was 79 years of age, and was still usefully employed. His eulogy will appear in the journal of the Lycæum of his department.

He has been succeeded by Thulis, who has long been assistant director of the observatory. The latter made proselytes and pupils C. Planpain and C. Degrand; but they have both left us, to the great loss of astronomy.

In the month of December 1800, Matteucci died at Bologna: to him we are indebted for the last volumes of the Ephemerides of Bologna, which go as far as 1810. He has been succeeded by C. Ciccolini and Guglielmini, who promise new activity in the observatory, which Manfredi, Za-

notti, and Matteucci, have rendered interesting for a century past.

Chaligni died lately at Madrid; he had long made observations and calculations, by which he has been known with advantage as an astronomer.

M. Chevalier has died at Prague: he made useful observations at Lisbon in 1759, and at Brussels.

On the 8th of October, Gabriel de Bory died at Paris, aged 81: he undertook a journey to Spain in 1751, and in 1758 went to Portugal and Madeira to determine their position. His observations are in the Mémoires of 1768, p. 270, and in those of 1772, part ii. In the Mémoires of 1770 he gave a description of a portable observatory, and in the third volume of the *Savans Etrangers* an observation of the transit of Mercury in 1753. In 1751 he published a description of a marine octant: he diffused a taste for observations through the royal navy: being *chef d'escadre* and governor of the windward islands, he had means of contributing to excite emulation, and he always employed them. In 1765 he was therefore elected a free associate of the Academy of Sciences, and in 1798 member of the Institute.

The Academy and Institute have always been sensible how much need we have of enlightened fellow-labourers to improve our knowledge of navigation, the most difficult of all arts, and the most important of all sciences for the prosperity and greatness of states.

But the greatest loss sustained by astronomy is that of Joseph Beauchamp. He was born at Vezoul on the 29th of June 1752. His observations at Bagdad in Persia, and on the Black sea, were as laborious to him as they were important to us. He set out in 1795 as French consul for Mascate in Arabia; and he wrote to me on his departure as follows: "Remember my attachment to you and to astronomy." He left indeed, with some regret, a country and family who were dear to him: he is certainly one of the martyrs to astronomy. He set out for Constantinople on the 25th of September; we expected him with the greatest impatience, but he had scarcely arrived in Provence when he fell a prey to a malady of which he had not been properly cured. He died at Nice on the 19th of November 1801. Eight days before his death the section of astronomy had presented him to the vacant place in the Institute. I published a notice of his labours in the Moniteur of Dec. 15, 1801.

XXXVIII. *Analyſis of the Arſeniates of Copper and of Iron.* By RICHARD CHENEVIX, *Eſq. F.R.S. M.R.I.A.*

[Continued from p. 147.]

NO. VII. *Yellow hematitic copper ore**. (See page 4.)

One hundred parts of this ore, boiled with dilute nitric acid, left a yellowiſh white reſiduum, which weighed 17. Theſe 17, expoſed to a degree of heat ſufficient to volatilize the ſulphur, left 5, which were ſilica. The liquor from which this reſiduum had been ſeparated by filtration, upon being tried for all the different metals, and particularly for arſenic, afforded no traces of any thing but copper and iron. A copious precipitate took place by the affuſion of ammonia; the copper was rediſſolved by adding an exceſs; and then obtained by volatilizing that alkali, and boiling with potaſh, after the filter had ſeparated the iron already precipitated. The contents are,

Sulphur - - - -	12
Silica - - - - -	5
Copper, which I believe to be in the metallic ſtate	30
Oxide of iron - - - -	53
	100

In this ore, I believe, for the following reaſons, that the metals are in the ſtate I have marked. Firſt, the proportions in the ore announce it; for I always had an exceſs of weight in the total reſult, if I did not deduct ſuch a proportion of oxygen as might be contained in 30 parts of copper.

Secondly, there is a conſiderable diſengagement of nitrous gas.

Thirdly, the ore does not attract the loadſtone.

And, fourthly, the greater part of the iron (but none of the copper) is diſſolved in muriatic acid, forming a green muriate of iron, without diſengagement of hydrogen gas.

No. VIII. *Gray vitreous copper ore.* (See page 6.) There are many intermediate ſtates between this ore and the yellow hematitic copper ore; but they are not fair objects of chemical analyſis, being merely mixtures of both kinds, in different proportions. The mineralogiſt, indeed, may dwell upon them, as intereſting in ſtudying the products of nature, but they are unſatisfactory ſubjects for the chemiſt.

* This and the following (No. VIII.) being the matrices upon which the arſeniates of copper and of iron are generally found, I thought it right to give an analyſis of them alſo.

Gray

Gray vitreous copper ore, when obtained in its greateſt purity, is by many degrees the richeſt cupreous pyrites known in nature; and, in the large way, the metal may be extracted by the eaſieſt proceſſes. 100 parts of this ore, in dilute nitric acid, left 12, which were ſulphur. Ammonia, poured in exceſs into the nitric ſolution, rediſſolved, with the exception of 4, the whole of the precipitate which it had formed; the 4 were iron; and, from the ammoniacal liquor, 105 of black oxide of copper, equal to 84 of metallic copper, were obtained by evaporation, and then boiling with potaſh.

Sulphur - - - -	12
Copper - - - -	84
Iron - - - -	4
	100

Although I have mentioned, in the preceding ſtatements, only a ſingle analyſis of each ſpecimen, it is by no means to be imagined, that ſo ſmall a number would be ſufficient to ſatisfy inquiry. None of the above reſults have been taken into account, unleſs confirmed by frequent repetition; and the probationary experiments have been diverſified, as much as lay in my power, by many different teſts, and various chemical reagents.

With regard to the colour of ſome ſpecimens of arſeniate of copper, it is eaſily to be accounted for upon chemical principles. The miſtake under which we have long laboured, that the green is the real oxide of copper, has happily been rectified by M. Prouſt*. He has proved it to be a particular ſubſtance, (to which he has given the very improper name of *hydrate of copper*†,) endowed with peculiar properties, and compoſed of the brown oxide, and of water, in a ſtate of combination. From his experiments, and from what I myſelf have ſeen, I am inclined to draw the concluſion, that we have never yet obtained by art any real ſalt of oxide of copper. In examining, for inſtance, ſulphate of copper, we find it to afford blue cryſtals; and to contain a known quantity of water of cryſtallization, and of what we formerly called the oxide. But that oxide ſtill retains a quantity of water, of which when it is deprived it paſſes to a very dark brown, and changes its chemical nature and properties.

* Annales de Chimie, vol. xxxii. p. 26.

† Copper is not the only metal capable of a ſimilar combination. Cobalt, nickel, and uranium, enjoy the ſame property. This may, in ſome meaſure, explain the change of colour which the liquid muriates of ſome of theſe metals undergo by gentle heat; it may likewiſe throw ſome light upon the ſympathetic ink of cobalt.

If, upon that brown oxide, a ſufficient quantity of dilute ſulphuric acid is poured, it yields a blue ſalt, but in a proportion greater by about 24 per cent. than if the green ſubſtance had been employed. I imagine, therefore, that the firſt operation of this brown oxide is, to aſſume the quantity of water neceſſary to conſtitute a hydrate; and, that the combination of ſulphuric acid takes place, not between the oxide, but between the hydrate and that acid, to form a ſalt, which, when cryſtallized, has taken another portion of water in the act of cryſtallization. It is a well known fact, that there is a ſtate of concentration, when an aqueous ſolution of muriate of copper, gently heated, will change from a blueiſh green to a beautiful brown, which, upon cooling, or by the affuſion of water, reſumes its former tinge. This brown liquor is probably a ſolution of muriate of copper; while the blue liquor, like every green or blue ſolution of a cupreous ſalt, is a combination of the acid and the hydrate, or (as we ſhould ſay in this caſe) a muriate of hydrate of copper. It is true, I have not been able to produce, ſo often as I could wiſh, this change of colour. I can, however, adduce the following inſtance, as being much in favour of my opinion.

It is evident that oxide of copper (for ſo I ſhall henceforth call the brown ſubſtance) has a very ſtrong affinity for water; becauſe the fixed alkalis, (unleſs when boiled upon it,) and their carbonates, all of which eaſily decompoſe the ſalts of copper, cannot diſpoſſeſs the hydrate of copper of its water.

This led me to imagine that I could perhaps, by fire, diſſipate not only the water of cryſtallization, but that contained in the hydrate; and leave the acid, if a potent one, ſtill in the ſalt. But I found that the affinity of the alkali, acting upon the acid, had, in the humid way, determined an order of combination not to be effected by heat; for even ſulphuric acid was expelled before the water of the hydrate could be completely diſſipated. Upon reflecting on the fixity of acids, I could find none ſo proper for this experiment as the phoſphoric. I therefore prepared ſome artificial phoſphate of copper, by precipitating the nitrate of that metal by phoſphate of ſoda. When waſhed and dried, it was in the form of a fine blueiſh green powder, among which many cryſtals were diſcernible almoſt to the naked eye.

One hundred parts of this, expoſed to a gentle red heat, became of a much paler green, but paſſed entirely to brown when the temperature was ſufficiently elevated. I had then a brown phoſphate, not of hydrate, but of oxide of copper, and from which no acid had been volatilized. Its loſs of weight was wholly from the water which had been expelled,

and

and amounted to 15,5. Its other proportions I found, by further analyſis, to be 35 of phoſphoric acid, and 49,5 of oxide of copper. It is not, however, to be concluded from this, that there are really 15,5 of water of cryſtallization, in blueiſh green phoſphate of copper. We muſt recollect that it is a phoſphate of hydrate of copper, and that 49,5 of oxide demand 12 of water to exiſt in that ſtate: 3,5 therefore are the amount of the water of cryſtallization; and its order of union may, with more propriety, be thus ſtated:

Oxide of copper	49,5	forming hydrate of copper -	61,5
Water -	12		
Phoſphoric acid - - - -			35
Water of cryſtallization - - -			3,5
			100,0

And this is the order which ſhould be adopted in the ſtatement of all analyſes of ſalts of copper.

I could eaſily produce, by the ſame method, a pale green, or a brown arſeniate; and, in nature alſo, the colour of the ore accurately correſponds with the proportion of water, as may be ſeen by comparing together any of the foregoing analyſes.

Having thus convinced myſelf, by analyſis, that copper is found in nature united with arſenic acid in different proportions, I next wiſhed to aſcertain whether art could effect ſimilar combinations. For this purpoſe, I poured into arſeniate of ammonia, a ſolution of nitrate of copper. The metallic arſeniate was immediately precipitated in cryſtalline grains, of a blue colour, rather more intenſe than the phoſphate already mentioned; and the liquor, which remained blue, was decanted. The colour which this latter retained, I imagined, was due to the preſence of a greater quantity of nitrate of copper than was neceſſary to precipitate, from its alkaline baſis, the arſenic acid combined with the copper. After a partial evaporation, I poured in alcohol; and found, to my ſurpriſe, that the conſequence was another precipitation, which was much increaſed by allowing the liquor to remain. Cryſtals ſtill more rich in colour than the former, and very evidently rhomboidal, even to the naked eye, were gradually formed. Imagining there muſt be ſome eſſential cauſe of the greater ſolubility of the one than of the other, I reſolved to examine them apart.

One hundred parts of the firſt of thoſe precipitates, expoſed to a low red heat, loſt 22. Boiled with potaſh, there remained undiſſolved a blackiſh brown powder, which, well waſhed and dried, weighed 50. The ſupernatant liquor, ſaturated with

nitric acid, and evaporated, was precipitated by nitrate of lead. Upon filtration, 82 were left, which indicate 27 of arſenic acid. Therefore this arſeniate of copper contained,

Copper - - -	50
Arſenic acid - -	27
Water - - -	22
	99

The ſecond artificial compound, which was mentioned above, was evidently more ſoluble than the latter; and analogy might lead us to ſuſpect; in arguing from the generality of ſalts the baſis of which is ſuperſaturated, that it contained an exceſs of acid. It was analyſed in the ſame manner as the laſt, and afforded,

Oxide of copper - -	35
Arſenic acid - - -	39,5
Water - - - -	24
	98,5

Thus then have we two artificial arſeniates of copper, one of which intimately correſponds with one of thoſe which we have recognized among the productions of nature. The other poſſibly will be found, but we are not yet in poſſeſſion of it; for I ſhall preſently mention the reaſon why No. I., although containing 39 of acid, cannot fairly be eſteemed as ſuch. I have not yet been ſo fortunate as to form the other combinations, but do not doubt that art may one day ſucceed in obtaining them.

REMARKS.

Before I conclude this ſection, which hitherto has had for its object a particular account of certain kinds of copper ore, as well as of their matrices, it may not be ſuperfluous to offer a few remarks, not foreign to the preſent ſubject, upon ſome methods generally uſed in the docimaſtic art. To prove the preſence of different ſubſtances in foſſils, is an object of delicate reſearch: but, to determine proportions with accuracy, is the moſt difficult operation of analytic chemiſtry, and often eludes inveſtigation. It is rather a pleaſing reflection to think that we are in the infancy of chemical exactneſs; and that we may ſee the day of improvement, when the errors which we now commit will require all the aid of ſelf-complacency to be in the leaſt excuſed. And it may be of more real utility to ſtate with frankneſs, although we cannot account for them, thoſe anomalous appearances which ſo frequently occur, than to court the phantom of rigid accuracy, the reality

ality of which we can as yet, but in a few inſtances, be ſure we have attained. For every obſervation, however trivial, of this kind, will haſten the arrival of that moment when we ſhall be enabled to approach a little nearer to truth.

I have, for many reaſons, preferred boiling the nitrate of copper with either of the fixed alkalis, to the method generally recommended, which is, to precipitate all the copper from its ſolvent, by carbonate of potaſh or of ſoda; then, to rediſſolve in muriatic acid; and to precipitate, in the metallic ſtate, by a plate of poliſhed iron.

Firſt, when an alkaline carbonate is uſed, the precipitate is a carbonate of hydrate of copper; and this ſubſtance is ſoluble in an exceſs of the precipitant. I once evaporated ſome very beautiful blue liquor, obtained in an operation of this kind, and found a cryſtallized ſalt, which I became deſirous to examine. But, as the ſolution contained another ſalt, formed by the acid (which originally held the copper in ſolution) and the alkali employed, I found it neceſſary to form ſome hydrate of copper directly for the purpoſe.

Some hydrate of copper was therefore prepared, by decompoſing the nitrate of that metal by a very dilute ſolution of potaſh, and well waſhing and drying the precipitate: it was that fine powder formerly well known as the oxide of copper. Some of this ſubſtance was thrown into a ſolution of carbonate of potaſh, through which a current of carbonic acid had been made to paſs for a long time, and they were then ſlightly heated together. One part of the hydrate became of the ſame colour as the real oxide; the other was diſſolved, and the liquor was converted into a greeniſh blue ſolution. Thus, one part of the hydrate had yielded its water to the other, in order to favour this quadruple, or rather this double combination, of carbonate of potaſh and hydrate of copper: the liquor, when reduced, afforded a maſs, which, repeatedly rediſſolved and evaporated, with difficulty aſſumed any determinate form of cryſtallization. This ſalt is a carbonate of potaſh, holding a little copper. It is of a pale blue, and varies in colour according to the quantity of water of cryſtallization, and of metal. It is ſlightly deliqueſcent, and ſoluble in about three parts of water, at 60°, but requires much leſs water when the water is boiling. It cryſtallizes by cooling, much like carbonate of potaſh. It is ſoluble in a large quantity of alcohol. It loſes about 43 per cent. of carbonic acid, by ſolution in a ſtronger acid; and, prepared in the manner I have mentioned, contains no more than 5 per cent. of oxide of copper; although carbonate of potaſh, when poured into a ſolution of any ſalt of copper, appears

to

to retain a much greater quantity. This, therefore, is a ſufficient proof of the inaccuracy of attempting to precipitate copper from its ſolutions, by an alkaline carbonate; for carbonate of ſoda has, like carbonate of potaſh, the property of diſſolving and cryſtallizing, as a triple ſalt, with hydrate of copper.

In addition to this ſource of error, we may add the very uncertain operation of one metal upon the ſaline combination of another, whatever may be their affinity for oxygen. Indeed I have ſo often remarked this very great uncertainty, that I was pleaſed to find the obſervation had occurred to Mr. Prouſt; and I have conſtantly found that more dependence can be placed upon the method I have adopted, than upon any attempts to precipitate the metals by each other.

With regard to efficacy and promptneſs, tin or zinc is preferable to iron; but, with any of the three metals, a phænomenon occurs, for which I have not been able to account in any manner the leaſt ſatisfactory. As the effects are more ſtriking with zinc than with the other metals, I ſhall enter into particulars reſpecting the uſe of that metal only.

When a bit of zinc (or tin, or iron,) is immerſed in a ſolution of muriate of copper, a precipitation begins, and all the copper is finally ſeparated in a few hours*: but, if a little muriatic acid is added to this ſolution, and the zinc then immerſed, a violent effervescence takes place; hydrogen gas is diſengaged, and, in leſs than two minutes, the copper is ſo completely precipitated in the metallic ſtate, that neither ammonia, nor even ſulphurated hydrogen, can diſcover any veſtige of its preſence.

It would appear from this, that hydrogen is, in this caſe, the real reductive and precipitant of the copper. Yet, how can we reconcile the evident contradiction, that, in one caſe, zinc with muriatic acid will decompoſe water, that is, that zinc and muriatic acid will attract oxygen more ſtrongly than hydrogen can, yet that, in the other caſe, hydrogen, whoſe affinity for oxygen is weaker than that of zinc and muriatic acid, will be more ſpeedy and powerful than zinc and muriatic acid, in attracting that oxygen from copper? Again, how is it poſſible that zinc and muriatic acid will, in preference to taking oxygen immediately from the oxide of copper, decompoſe water, the hydrogen of which will unite with the oxygen of the oxide of copper, again to become water, which it originally was? All this appears to me, I confeſs, as contradictory as to ſay, one is leſs than two, two are leſs

* If any quantity of neutral ſalt is preſent in the ſolution, the precipitation is much retarded, and is ſeldom complete.

than

than three, yet three are much leſs than one. This opinion, that hydrogen is really the reductive, is the more extraordinary, as it is not founded on the ſingle experiment above mentioned.

If a bit of zinc (or tin, or iron,) is thrown into a ſolution of oxide of arſenic in water, no change of any kind will be effected, even after a conſiderable time: but the inſtant that muriatic acid is added, effervescence and precipitation commence; a few minutes ſuffice to obtain all the arſenic in its metallic ſtate. It may be objected here, that muriatic acid (as muriatic acid) produces ſome hitherto unknown modification in the order of eſtabliſhed affinities. This objection is not plauſible; and I can adduce ſo clear an anſwer to it, as to ſhow that it is of very little moment. If aqueous arſenic acid is uſed inſtead of the above ſolution, the ſame phænomena of effervescence and precipitation enſue as when muriatic acid had been uſed in the former caſe; and that preciſe quantity of metallic arſenic is thrown down, which can yield ſufficient oxygen to the zinc to combine with the remaining acid undecompoſed. The precipitate which is formed, is a mixture of metallic arſenic with arſeniate of zinc; and theſe may be ſeparated by muriatic acid, which will diſſolve the metallic ſalt, without acting upon the arſenic.

If, inſtead of muriatic acid, ſulphuric acid is uſed, the ſame phænomena take place, in a leſs degree. But, if the experiment is tried with nitric acid, there is no diſengagement of hydrogen gas; and the metals effect a precipitation with much leſs rapidity and certainty than in the former caſe.

It is very true, that hydrogen, in its naſcent ſtate, may have properties with which we are yet unacquainted; and may determine combinations, which it can in no other ſtate produce. But the decompoſition of water, in the firſt inſtance, in order that a new recompoſition may, at that moment, be effected by the ſame agents, is in itſelf ſufficiently paradoxical.

The facts laſt mentioned are ſomewhat analogous to, and ſeem even confirmative of, a theory propoſed ſome time paſt by Mrs. Fulhame; but I ſhall withhold my full aſſent, both to her explanation and to any I could give, until theſe evident contradictions can in ſome way be further elucidated.

In the analyſis of every ore in which the metal is combined with ſulphur, I have found much variation in the quantity of the latter which may be obtained, even in experiments upon the ſame ſpecimen. If nitric acid is uſed not ſufficiently ſtrong, part of the ore remains untouched, and will require ſubſequent treatment, always diſadvantageous in delicate

delicate operations. If the acid is too ſtrong, a great part of the ſulphur is converted into ſulphuric acid; ſo that, in either caſe, there is room for error. I thought that, to avoid this, (except in caſes where any metal which, with ſulphuric acid, forms an inſoluble ſalt, was preſent,) ſtrong nitric acid might be uſed, and all the ſulphur converted into acid. If potaſh, ſoda *, or ammonia, are uſed as precipitants of the different metals, the quantity of ſulphur may be eaſily aſcertained. I took a given weight of ſulphur, and converted it into ſulphuric acid by means of nitric acid. I then neutralized and evaporated it. Nitrate of barytes, poured in, gave a precipitate which, in one experiment, indicated a proportion of ſulphur equal to 14,4, and in another to 14,6, contained in one hundred of ſulphate of barytes. A difference ſo trifling need not be regarded. According to M. Lavoiſier, ſulphuric acid contains 71 of ſulphur, and 29 of oxygen; and, according to the ſynoptic tables of M. Fourcroy, ſulphate of barytes contains 33 per cent. of ſulphuric acid: therefore, by this calculation, one hundred of ſulphate of barytes contain 23,43 of ſulphur, inſtead of 14,4, or 14,6. I do not pretend to account for ſo great a difference in theſe reſults †; but that very difference led me (by exciting me to doubt thoſe which I had obtained, and inducing me to frequent repetition,) to a more poſitive conviction of the proportions I have mentioned. M. Lavoiſier obtained his proportions by combuſtion; and, admitting even that nothing was loſt, it muſt have been rather difficult to obtain the ſulphuric acid in a ſtate proper to value the quantity. Indeed I do not know of any direct experiments which prove, in a ſatisfactory manner, that we have ever obtained that acid perfectly free from water; unleſs when combined with an earth, or an alkali, in ſome ſalt, and that ſalt calcined in a very ſtrong red heat.

To aſcertain the quantity of metallic arſenic in miſpickel, arſenical pyrites, &c. the moſt advantageous method is, to acidify it by nitric acid, and then to combine it with oxide of lead. This arſeniate of lead (containing, as was before ſaid, 33,2 per cent. of acid,) may be eſtimated to contain 22 per cent. of metallic arſenic. If both ſulphur and arſenic are preſent, lead may be equally ſerviceable, after both have been acidified; for ſulphate of lead is not materially ſoluble in any acid; whereas, on the contrary, arſeniate of lead is very much ſo.

* See note in page 144.

† I was particularly cautious in aſcertaining that, during that experiment, no ſulphureous acid had been produced, the formation of which would have eaſily accounted for any difference.

When

When copper and iron are to be ſeparated, one ſingle affuſion of ammonia will not always ſuffice. That two may be ſometimes neceſſary, is an objection to the method I propoſe for the ſubſequent ebullition with potaſh. But, when I uſe that of precipitating the copper by iron, it requires no previous precipitation by any alkali. It is ſufficient to add muriatic acid to the original ſolution of the ore in nitric acid, and evaporate to dryneſs. The nitric acid is diſlodged from the oxide of copper, and the muriatic takes its place. If a ſingle evaporation is not ſufficient, a ſecond (for the operation is very ſhort, and cauſes no loſs upon filters, &c.) may be attempted; and when the iron uſed for the purpoſe of revivifying the copper is put in, the liquor may be made to boil; by which means the proceſs is rendered much more certain and expeditious. Some iron will neceſſarily be diſſolved, and the quantity muſt be noted. The liquor, which contained muriate of copper and of iron, now contains only the latter. Boiled with a little nitric acid, it will become red; and then ammonia, or potaſh, will give a red precipitate, which, well waſhed and dried, will repreſent 61 per cent. of metallic iron. All theſe metals having thus been precipitated, no conſtituent part of the ore, except the ſulphur, which, in the firſt treatment, had been converted into ſulphuric acid, is contained in the liquor; and from it, when neutralized, this latter may be precipitated by nitrate of barytes, which will repreſent 14,6 per cent. of ſulphur. The abſolute neceſſity of conſtantly uſing pure alkalis, in this method of analyſis, is too evident to be inſiſted upon.

GENERAL VIEW OF THE FOREGOING ANALYSES.

In taking a retroſpective ſurvey of the experiments above related, upon the various natural arſeniates of copper which we have examined, we ſhall find,

Firſt, That natural arſeniate of copper exiſts in three different ſtates of combination; the firſt containing 14 per cent., the ſecond 21 per cent., and the third about 29 per cent. of acid.

Secondly, That each of theſe may contain different proportions of water, either as conſtituting a hydrate, or as water of cryſtallization.

Thirdly, That, upon loſing its water, arſeniate of copper will paſs from blue to pale green, and finally to brown, as in No. I.

Fourthly, That No. I. is the only real arſeniate of copper, all the others being arſeniates of hydrate of copper.

Fifthly, That No. I. is not to be admitted as an arſeniate

of

of copper containing 39,7 per cent. of acid. For, if we put it on the ſame footing with the others, in admitting a due proportion of water into its compoſition, we ſhall, by calculation, reduce it to that claſs containing 29 per cent.

Sixthly, That in beginning with that kind which contains the leaſt quantity of acid, and riſing progreſſively to that which contains the greateſt, we ſhall find the order to be thus:

No. VI. contains	-	14 per cent.
No. V. - -	-	21 per cent.
Nos. I. III. and IV.	-	29 per cent.

No. II. ſeems to be a particular ſpecies. It conſiſts of a much greater proportion of oxide, with a leſs quantity of water, (and this its external colour announces,) combined with nearly the ſame proportion of arſenic acid. Indeed, if certain characters did not ſpeak ſo ſtrongly in favour of this diviſion, I ſhould not have heſitated to claſs it with the laſt-mentioned kinds. But it is found in many ſtates; which ſeems to indicate, that the water is by no means in the ſame degree of intimate combination that it is in the others; and this alone may ſerve to diſtinguiſh it to the eye of the mineralogiſt.

If, to the above natural arſeniates, is added the ſecond artificial arſeniate, we ſhall have another proportion of acid, at the rate of 40 per cent. Here then we have two ſimple ſubſtances combined in four different proportions, and producing ſeven diſtinct combinations.

But, what is not the leaſt to be admired, is the wonderful accordance in the order which two ſciences, operating with very different inſtruments, have allotted to the ſame ſubſtances. By that, not only the ſagacity of Nature becomes very ſtriking; but, from the acknowledged accuracy of one method of inveſtigation, the reliance to be placed upon the other is rendered more conſpicuous; and each receives additional ſtrength and confirmation. Chemiſtry has long been in the habit of aiding the ſcience of mineralogy, of which it laid the foundation; but it was not till lately that cryſtallography could form a judgment of its own, much leſs confirm the truth of the ſource from which it ſprung.

Section II.

Arſeniates of Iron.

The arſeniates of iron remain now to be examined. Included, formerly, among arſeniates of copper, they have been ſeparated from them upon the authority of chemical analyſis. For, although to recogniſe, by external character and form in all their modifications, ſubſtances already known, is particularly

ticularly the province of cryſtallography; yet he who would expect that it ſhould declare the nature of thoſe ſubſtances which it beholds for the firſt time, would exact more than it ever has promiſed, or ever could perform. Among foſſils, it may claſs, and find new ſpecies; but chemical analyſis is the baſis of all arrangement among metallic ores. In them, to ſeparate, is the taſk of the one; to aſſign a place, is the buſineſs of the other.

Cupreous Arſeniate of Iron *.

One hundred parts of this arſeniate, expoſed to a low red heat, loſt 12, which were pure water. Nitric acid was poured upon the reſiduum; and, finding that it was diſſolved with difficulty, the ebullition was continued during ſeveral hours. The liquor was then filtered. Sixty parts, which ſhall preſently be examined, remained undiſſolved. Into this filtered liquor, nitrate of lead was poured, which occaſioned a precipitate, as uſual; but the operation was diſcontinued until I ſhould obtain all the arſeniate of copper which I imagined to be contained in the ore. For this purpoſe, I had recourſe to the 60 parts mentioned above. They were in the form of a greeniſh gray powder, very hard and gritty, which had every appearance of ſilica, contaminated by a ſmall portion of copper interpoſed between the molecules of that earth. I reſolved to treat it in the ſame manner as all ſiliceous ſtones, and proceeded to boil it with potaſh.

In leſs than three minutes it became of a very red brown, from the greeniſh gray which it originally was; and ſeemed conſiderably attenuated in its particles. The liquor was decanted, and examined. It was found to contain arſenic acid; and the precipitate, which had reſiſted the action of the potaſh, was proved to be a mixture of iron and copper.

Theſe preliminary experiments were ſufficient to indicate a ready method of analyſis. 100 parts, boiled with potaſh, immediately became of a deep reddiſh brown. The liquor was ſeparated from the reſiduum by filtration; and, after the uſual neutralization, evaporation, and affuſion of nitrate of lead, (all of which operations were detailed in the firſt part of this paper,) gave a precipitate correſponding to 35,5 of

* This ſpecies had been mentioned by Mr. Prouſt, but in a manner which, as it was a new ſubſtance, and demanded particular attention, does not give all the ſatisfaction which that chemiſt generally affords. No doubt the ſcarcity of the ore prevented his making every neceſſary reſearch; and I may deem myſelf fortunate in having been ſo near the ſpot in which it is found. My friend Mr. Hatchett very obligingly gave me a ſpecimen of this ore, which he had received from Dr. Pallas, who had brought it with him from Siberia, where it had been found.

arſenic

arſenic acid. The firſt reſiduum weighed 53. Diſſolved, as far as they could be, in muriatic acid, there remained 3, which, upon examination, were found to be really ſilica. Ammonia, poured in exceſs into the muriatic ſolution, redisſolved 22,5, which were copper; and 27,5 of iron remained behind. The proportions were,

Silica - - -	3
Arſenic acid - -	33,5
Oxide of iron - -	27,5
Oxide of copper - -	22,5
Water - -	12
	98,5

None of theſe experiments were ſufficient to determine, whether this ore is in the ſtate of a triple ſalt, or merely a mixture of two arſeniates. As, in a ternary combination, the proportion of acid might vary, it cannot be juſtly called in to aid us in our inquiry. The ſolubility of one part of the ore being much greater than that of the other, and in different quantities of each ſalt, incline more to the opinion that it is but a mixture.

Simple Arſeniate of Iron.

This arſeniate, expoſed to any degree of heat, gave but an unſatisfactory reſult, with regard to the quantity of water. The arſenic acid is volatilized from this ore with peculiar facility, for which I ſhall attempt hereafter to account.

Some ſubſequent experiments, however, have induced me to fix the quantity of water at about 10,5.

One hundred parts, boiled with potaſh, left 58,5. The liquor, treated, as uſual, by nitrate of lead, gave 31 of arſenic acid. The 58,5 left four, which muriatic acid could not diſſolve, and which were ſilica. Ammonia diſſolved 9; and there remained 45,5 of iron. This analyſis preſents the following reſult:

Silica - - - -	4
Arſenic acid - - -	31
Oxide of iron - -	45,5
Oxide of copper - -	9
Which will leave for water	10,5
	100,0

This ore appears to be a pure arſeniate, mixed accidentally with a little copper; as ſome of the copper arſeniates caſually give traces of iron. This is the kind mentioned by Mr. Klaproth as an arſeniate of copper, and the firſt known under

under that denomination. Heating it on charcoal, before the blowpipe, he perceived a ſmell of arſenic, and, at length, obtained a metallic button, which was found to be copper. That there is copper in this ore, is evident from analyſis. But the mere arſenical ſmell was not a ſufficient ground to aſſert that it contained arſenic acid; for this metal might, with as much probability, have been in any other ſtate. If, indeed, that very accurate and able analyſt had, upon trying the ore with the blowpipe in a platina ſpoon, perceived no fumes, he might then have concluded that the arſenic muſt be in the ſtate of acid, and that charcoal was neceſſary to operate a partial reduction, to which the arſenic owed its volatility and its ſmell. But no ſuch experiment is reported.

It is alſo rather extraordinary that Mr. Gmelin ſhould have taken this ore out of the claſs of arſenical ores, and left it as an unknown ſpecies of copper; when, in fact, it is an arſenical ore, but not an arſenical ore of copper.

I examined ſome cryſtals which are commonly attached to the ſpecimens of this ore. They were thoſe which, according to M. de Bournon, are in a ſtate of decompoſition. By this ſpontaneous decay, they become of a deep browniſh red, not unlike the ſubſtance called colcothar; but they ſtill retain their cubic form. They contain a little acid and water, owing, perhaps, to their having eſcaped from total decompoſition. The ſame theory that accounts for the difficulty of aſcertaining the quantity of water will account for the red colour they thus aſſume.

When green ſulphate of iron is expoſed in a crucible to a red heat, it is well known that ſulphureous acid is diſengaged in great quantities; and that, if the operation is continued long enough, there remains a red powder. In this caſe, the green oxide of iron has taken up oxygen from the acid; and this latter has been partly decompoſed, and almoſt totally volatilized. Now, in the ſpecies here ſpoken of, the iron, as in the green ſulphate, is in the ſtate of green oxide; therefore capable of receiving an additional portion of oxygen. But arſenic acid will, at a high temperature, loſe a part of its oxygen, and, retrograding to the ſtate of white oxide, will be volatilized; and ſtill more eaſily will thoſe changes take place, when oxide of iron, ready to receive, and arſenic acid, ready to yield oxygen, are in contact. A leſs degree of heat, therefore, will ſuffice to drive off this acid, from green arſeniate of iron, than from arſeniate of copper. But we muſt not from this conclude, that the affinity of the latter metal for arſenic acid is ſuperior to that of the former; for, the attraction of green oxide of iron for oxygen, and of caloric for

white oxide of arſenic, determines a new order of diveſling affinities.

But moſt of the mineral acids that have been tried have been found capable of uniting with iron in two ſtates; in the ſtate of green oxide, and in that of red oxide, the reſiduary powder above mentioned. I was deſirous to know whether I could, in any manner, imitate, by art, the laſt natural products I have examined, as I had already imitated, in ſome degree, an arſeniate of copper. For this purpoſe, I decompoſed green ſulphate and red ſulphate of iron, by arſeniate of ammonia; and, having well waſhed and dried the precipitates, proceeded to examine them.

The green arſeniate was acted upon by heat, in the ſame manner as the natural one, and exhibited the ſame appearances. By the uſual methods, I found its proportions to be,

Oxide of iron	-	43
Arſenic acid	-	38
Water -	-	19
		100

This is not the ſame proportion of acid that is contained in the natural arſeniate; however, I ſtate them both as I found them. The other artificial arſeniate, which is of a pale greeniſh red, afforded,

Red oxide of iron	-	36,5
Arſenic acid -	-	41,5
Water - -	-	20,0
		98,0

Theſe ſalts agree with the generality of the known ſalts of iron; all of which contain a greater quantity of oxide, as the oxide itſelf contains leſs oxygen.

By boiling with nitric acid, it was eaſy to convert the green arſeniate of iron into the red; and ſuch is the caſe with all the ſalts of green oxide of iron. As, during the courſe of theſe experiments, I had occaſion to make ſome remarks upon the divers habitudes of this metal, which, as far as I know, have not all been obſerved, I ſhall terminate the whole of theſe analyſes (as I have already done that part of them which treats of arſeniate of copper in all its known varieties) by ſimply ſtating what has occurred to me.

I happened to boil ſome muriatic acid upon a greater quantity of iron than the acid could diſſolve. I found a perfectly limpid and colourleſs liquor remain, which, nevertheleſs, was a ſolution of muriate of iron. This colourleſs liquor being decompoſed by arſeniate of ammonia, the precipitate was

was of a greeniſh white, and ſoluble in a great quantity of water; but, paſſing to a much deeper green, quickly fell to the bottom. A pruſſiate or a gallate poured into the ſaid ſolution, occaſioned no change till it had ſtood a conſiderable time expoſed to the contact of the air. By potaſh, and by ſoda, a white precipitate was thrown down, which quickly aſſumed a green tinge; and that tinge increaſed ſo much as to become a very deep graſs green in a few minutes. Ammonia occaſioned a white precipitate, which was rediſſolved by an exceſs of the precipitant. The ammoniacal ſolution aſſumed the ſame greeniſh tinge, and ſpeedily depoſited an oxide of iron, which was firſt of a deep green, but inſtantaneouſly became black, with ſome yellowiſh ochrey particles on the ſurface. If, however, theſe precipitations were effected in a bottle well ſtopped, and defended from the contact of the atmoſphere, no change of colour took place; and that portion which was diſſolved by an exceſs of ammonia remained in the ſolution. In endeavouring to diſtil, or to evaporate, the water of this colourleſs ſolution, in order to obtain cryſtals, it became of a light green, the intenſity of which augmented, as the diſtillation was continued. I could not, therefore, hope to procure this ſalt in a cryſtalline form. Hence, it is evident that we have a white muriate of iron, which, as well as the oxide it contains, is very ſuſceptible of aſſuming an addition of oxygen; for to that alone I attribute the precipitation cauſed in a ſolution of that ſalt, into which the different reagents above mentioned had been poured; a precipitation which did not take place till after it had been expoſed in a ſituation where it could abſorb the quantity of oxygen neceſſary to produce a change in its principles.

This ſolution of white muriate of iron, when expoſed to the air, becomes green, and is then in the ſtate of green muriate, well known. At a certain degree of oxidation, I have obſerved the precipitate formed, to be ſoluble in the carbonates of potaſh and of ſoda, and ſtill more ſo in that of ammonia; but, upon abſorption of oxygen, to be quickly abandoned by them, and then to fall to the bottom, in a blackiſh powder mixed with yellow. If into a ſolution of green muriate of iron nitric acid is poured, both liquors being cool, and not too concentrate, the muriate will become of a blackiſh brown, not unlike malate of iron. Precipitated by the alkalis, it yields a black powder, no longer ſoluble in them, but which reſembles, in every reſpect, common black oxide of iron.

If this green muriate of iron is further expoſed to the air,

the liquor becomes red, but ſtill gives a blackiſh green precipitate; but, if it is boiled with nitric acid, it then is converted into a red liquor, which yields a red precipitate, by all the alkalis and earths capable of cauſing a precipitation. From theſe obſervations, upon the different combinations of iron with oxygen, and of oxide of iron with muriatic acid, ſome concluſions may be drawn intereſting to mineralogy as well as to chemiſtry. The variety of colour in many ſtones in which iron has been found, is a fact which, although we cannot deny our aſſent to direct experiments, has never been accounted for in a ſatisfactory manner. In white, green, yellow, black, red; in a word, in foſſils of every colour, iron, with ſometimes the help of manganeſe, and lately of chromium, has been regarded as the colouring matter of every ſhade; but it ſeems almoſt paradoxical, that the ſame ſubſtance ſhould aſſume and communicate ſo many tints. In mica, kaolin, amianthus, aſbeſtos, rock cryſtal, and all white ſtones, I believe it to exiſt as the white oxide; and that ſtate is its firſt degree of combination with oxygen. In them, this metal is not very abundant; for, if it were, I have ſome reaſon (as ſhall be proved immediately) to think that they would obey the loadſtone ſtrongly. In lapis ollaris, ſerpentines, and many green ſtones, we have the green oxide; and moſt of theſe are magnetic; nay, as Mr. Humboldt has obſerved, ſerpentines enjoy the property of polarity. And thence I conclude, that the rarity of this metal, in the firſt claſs, prevents them from participating that quality. This I believe to be its ſecond ſtage of combination with oxygen. In the ſtate of black oxide it is frequently found, and is too well known to need further comment. I believe this to be its third ſtage of oxidation. But there is a red liquid muriate, which gives a very dark greeniſh precipitate, approaching to dark brown. What the ſtate of that precipitate is, I have not yet determined; but I imagine it to be a mixture of black and red. Brown and yellow oxides, I am confident, are mixtures of ſimple oxides, and neither of them is an oxide *ſui generis*. The red oxide is the extreme of oxidation, and affords many beautiful colours in nature and in art.

It is much to be deſired, for the advancement of analytic chemiſtry, that experiments upon the proportions of oxygen with which metals are capable of uniting, under different circumſtances, and upon the combination of thoſe oxides with all the known acids, together with many others of their properties, would attract the notice, and engage the labours, of accurate manipulators. Experiments of this kind have been deſpiſed, from an idea of their reſembling a mere mechanical employment;

employment; but, so far is that from the truth, they may justly be considered among the most difficult problems of chemistry; and it is only from the rigid and constantly similar results of such experiments, that we can hope to attain an intimate knowledge of the principles with which nature has originally operated.

[To be continued.]

XXXIX. *Comparison of the Definitive Metre with a Standard of the English Measures, carried from London to Paris by* M. A. PICTET, *Professor of Philosophy at Geneva* *.

AS the measurement of the earth, and researches respecting the figure of it, had, at various times, and in different countries, during the course of the 18th century, been an object of the labours of the most distinguished philosophers †, when an idea was lately conceived in France of deducing from the dimensions of our globe a standard of unity, to which every thing susceptible of measurement or weight could be referred, it was necessary to make an effort proportioned to the importance of an enterprise become a national concern. In the midst, therefore, of a long and destructive war, and amidst a thousand difficulties of every kind, a series of triangles was established between Dunkirk and Barcelona, comprehending the tenth part of an arc of the meridian extending from the equator to the pole, that is to say, the fourth part of the whole circumference of the globe; and the ten millionth part of this arc so determined was adopted as representing the unity of the whole metric system. This plan was executed with standards composed of substances capable of resisting the influence of the weather; and by then establishing, as was carefully done, the exact ratio of the length of the metre to that of a common pendulum which swings seconds, on the borders of the sea, in a given latitude, the determination of this unity was rendered independent of any catastrophe that might alter or destroy its types, in the construction of which all those resources presented by philosophy and the arts, now brought to a very high degree of perfection, were employed.

On the other hand, geodesian operations, designed to fur-

* From *Bibliotheque Britannique*, by professor Pictet, No. 148.

† Swedish astronomers are now employed in again measuring a degree of the meridian, which was measured in Lapland under the polar circle by the French academicians above sixty years ago.

nish materials for a new map of Great Britain, were undertaken, about 25 years ago, in England, and have been carried on since that time. These labours, begun by the late general Roy, have been conducted with a great deal of sagacity and precision, and the results of them may afford interesting data in regard to the figure of the earth. A distinguished member of the Royal Society of London, Sir George Schuckburgh, has employed himself, with success, in researches calculated to establish the precise length of the standards, which have served as a basis to all the geodesian measurements made in England.

It was therefore to be regretted that labours so analogous in their nature, carried on in two neighbouring countries, and susceptible of acquiring, by comparison, a new degree of interest, should remain unconnected for want of a real standard of the measures of one of these countries, viz. England. We experienced this regret in a very lively manner at different periods; and we can with truth assert, that if the hope of procuring this mean of comparison was not the only motive of the journey undertaken by one of us to England, it was a strong inducement towards it.

Our colleague, professor Pictet, when he arrived at Paris, took some steps to obtain an authentic metre, in order that he might submit it to examination before the Royal Society of London, of which he is a member; but, in consequence of the shortness of his stay in Paris, these steps were not attended with the desired effect. He, however, took advantage of a longer residence in England to procure from Troughton, an artist celebrated for his accuracy in constructing and dividing geometrical and astronomical instruments, a standard exactly similar to one he had constructed for Sir George Schuckburgh, and to which that philosopher referred the principal English measures in the memoir above mentioned. Our colleague also caused to be constructed by the same artist the comparative apparatus of Sir George Shuckburgh, consisting of two excellent microscopes, one of which is furnished with a micrometer that divides the English inch into ten thousand equal parts. On his return to Paris he took the earliest opportunity of laying these articles before the Minister of the Interior and the National Institute. This learned body named three of its members to proceed to a regular comparison of the definitive metre with the standard just mentioned. This labour, of a simpler nature than it might at first appear, employed the commissioners during five sittings of four hours each, and was conducted with all the care and attention that the nature of the object required.

C. Prony,

C. Prony, to whom theſe reſearches were particularly intereſting, as it is to him we are indebted for a French tranſlation of general Roy's memoir on the firſt trigonometrical meaſurements made in England, noted down the reſults; and it was in the cabinet of that learned man, and by the aſſiſtance of a comparative apparatus belonging to him, that the principal operations were made. He was ſo kind as to tranſmit to us an authentic copy of the report given in to the Inſtitute; a production thought to be ſo generally intereſting, as to be read in the public ſitting of the laſt quarter. He adds, "that this report will be ſoon followed by a memoir, in which he will give more circumſtantial details reſpecting the obſervations he made; together with a deſcription and drawing of his comparative apparatus." In the mean time we ſhall lay before our readers the report, aſſuring them that we have taken every poſſible care, in reviſing the proofs, to avoid typographical errors.

NATIONAL INSTITUTE.

Extract from the Regiſters of the Claſs; Sitting of the 6th Nivoſe, Year 10 (*Dec.* 27, 1801.)

A member, in name of a commiſſion, read the following report on a compariſon of the ſtandard metre of the Inſtitute with the Engliſh foot:

"C. Pictet, profeſſor of philoſophy at Geneva, ſubmitted to the claſs, in the month of Vendemiaire laſt, an intereſting collection of objects relating to the ſciences and the arts, which he procured during his travels in England.

"Among theſe objects was a ſtandard of the Engliſh meaſures on a braſs ruler, ſtrong and well poliſhed, about 49 Engliſh inches in length, 36 of which make the Engliſh yard, and divided along its whole length, by lines exceedingly fine, into tenths of an inch.

"This ſtandard was conſtructed, at the requeſt of C. Pictet, by Troughton, an artiſt of London, who has the well-earned reputation of dividing inſtruments with great accuracy: it was compared at London with another ſtandard made by the ſame artiſt for Sir George Schuckburgh, and it was found that the difference between theſe two ſtandards was no more than the difference between each of the diviſions, that is to ſay, quantities abſolutely inſenſible.

"The ſtandard of C. Pictet, then, ought to be conſidered as identical with that of Sir George Schuckburgh: a detail of the experiments which aſcertain the exactneſs of the latter, may be ſeen in the Philoſophical Tranſactions for 1798.

"C. Pictet had laid before the Inſtitute, together with

the above rule, divided into tenths of an inch, a comparer or instrument proper for estimating the small differences between the measures, constructed by the same artist, and of which the following is a short description:

"The comparer is composed of two microscopes with threads, which are placed in a vertical situation. The faces of the rule to be examined being horizontal, and at a convenient distance from each other, by making them move along a metal rod with divisions, one of the microscopes remains fixed near one of the extreme lengths of the point to be compared, and serves to assure the immobility of that point; the second microscope is towards the other extremity of the same length, and fixed also; but the frame which bears these threads may be moved by means of a micrometer screw the threads of which are equal to $\frac{1}{100}$th of an inch English; and the plate passed over by the index being divided into another 100 parts, gives $\frac{1}{10000}$th of an inch English for every division of the plate. By means of this arrangement, if the microscopes are first placed in such a manner that the intersections of their threads correspond to the extremities of any length intended to be compared with another which differs from it by a quantity less than 1-10th of an inch, the difference of the two lengths, by employing the micrometer, may be estimated in 10,000th parts of an inch.

"The threads placed in the focus are disposed obliquely in regard to the lines of division, so that a correspondence is known to have taken place when a line of division divides into two equal parts the acute angle formed by the two threads.

"We shall here observe, that, in the year 1785, Ramsden employed, in the like manner, two microscopes, adapted to an instrument he had invented to measure the dilatation of metals. General Roy gave a description of this instrument in the 75th volume of the Philosophical Transactions.

"C. Pictet offered to entrust the class with the English standard and the comparer above described, to enable them to determine the ratio of the metre to the English foot: the class accepted this offer with gratitude, and charged C. Legendre, Mechain, and myself, (Prony,) to join with C. Pictet in order to determine the ratio between the standard of the platina metre and the English foot.

"The following is a short account of the result of the labour undertaken on this subject at the house of Lenoir for a first comparison:

"The 49 inches of the English rule terminate at divisions traced out on one of the faces of that rule; and the standards

of platina and iron of the Inſtitute have no diviſion, the length of the metre being given by the diſtance between their extremities. This circumſtance prevented, on the one hand, the length of a metre from being taken immediately on the ſtandards with the microſcopes; and, on the other, did not permit the immediate eſtimation of the metre of the Inſtitute in Engliſh inches, by the proceſs employed for fixing the ſtandard of the new meaſures; a proceſs which conſiſts in reſting the extremity of the metal rule ſubjected to examination againſt the knee of a fixed gauge, and applying to the other extremity a ſliding knee, conſtructed for eſtimating the difference between the meaſures, the ratio of which is to be determined, or the identity of which is to be aſcertained.

"The artiſt Lenoir tried to ſurmount theſe obſtacles by employing a copper rule, a metre in length, cut at its extremities into a very ſharp bevel, in ſuch a manner, that this rule could be compared with the ſtandard of the Inſtitute according to the uſual method of extreme contact, and which being placed above the Engliſh rule, the edges of the bevel produced on the ſurface of that rule the effect of diviſions parallel to thoſe delineated upon it. By theſe means, the microſcopes could be uſed for eſtimating in inches, and ten thouſandth parts of an inch, the diſtance between theſe edges.

"By this method, the ſtandard metre of platina, and another metre of iron, both belonging to the Inſtitute, were compared with the Engliſh foot; the two former meaſures being conſtructed in ſuch a manner, that at the temperature of melting ice they are equal to each other and to the ten millionth part of a quarter of the meridian. It was found that at the temperature of 15·3 degrees of the centigrade thermometer the platina metre was equal to 39·3775, and the iron metre to 39·3788 Engliſh inches, meaſured on the rule of C. Pictet.

"But theſe firſt obſervations made known to the commiſſioners that the proceſs they employed might leave ſome uncertainty, in conſequence of the great difficulty of placing the interſection of the threads at the exact extremity of the bevel of the rule, which ſerved as a term of compariſon. A reflection or irradiation of light which took place at that extremity, prevented our clearly diſtinguiſhing whether the optic axis of the microſcope was exactly tangent to the ſmall ſurface which terminated that bevel.

"To remedy this inconvenience, one of the commiſſioners*

* This method, as ſimple as ingenious, was propoſed by C. Prony; and C. Paul of Geneva, who was accidentally preſent, carried it into execution; it was attended with complete ſucceſs.

propoſed

propoſed the following method, which was adopted:—It conſiſted in tracing out, on a ſmall ſlip or rule of metal, of the ſame thickneſs as the Engliſh rule, a very delicate line perpendicular to the length of the rule. This piece of metal was made to reſt againſt a fixed knee, and the microſcope with fixed threads was brought to the line above mentioned: the piece of metal containing that line was then removed, and the metre to be compared was put in its room, with one of its extremities reſting againſt the fixed knee, and the piece of metal placed at the other. It is here evident that the line traced out on the piece of metal was in this new poſition at the exact diſtance of a metre from its former poſition when reſting againſt the fixed knee; and, by making the threads of the ſecond microſcope correſpond with this line, the diſtance between the points of interſection of the two threads was exactly a metre. To eſtimate the metre in Engliſh inches, nothing then was neceſſary but to put into its room the divided rule, to place a line of one of its diviſions under that of the interſection of the threads which was fixed, and to eſtimate at the other extremity, by means of the micrometer, the fraction of diviſion, which with the whole number of theſe diviſions gave the length of the metre.

"The compariſon was repeated on the 4th of Brumaire at the houſe of one of the commiſſioners by the method here deſcribed; and after ſeveral obſervations, which correſponded in a very ſatisfactory manner, it was found that, at the temperature of 12·75 degrees of the centigrade thermometer, the platina ſtandard of the metre was 39·3781, and the iron ſtandard 39·3795 Engliſh inches.

"The two metres having been conſtructed to be equal at the temperature of freezing, the operation from which the preceding reſults were obtained may be verified by finding what the ratios would be at that temperature. For this determination we have the correct experiments of Borda, and the commiſſion of weights and meaſures on the dilatation of platina, copper, and iron, from which it reſults, that for one degree of the centigrade thermometer, platina dilates 0·00000856, iron 0·00001156, and copper 0·00001783; and from theſe data it is found that, at the temperature of freezing, the platina metre of the Inſtitute is equal to 39·38280, and the iron metre to 39·38265 Engliſh inches, meaſured on the ſtandard of C. Pictet.

"The difference 0·00015 between theſe two lengths, leſs than $\frac{1}{3\frac{1}{2}}$th of a line, or $\frac{1}{200000}$th of a metre, may be abſolutely neglected. The reſult of our labour therefore is, that, ſuppoſing the platina and iron ſtandards of the metre belonging

ing to the Inſtitute, and the Engliſh rule of C. Pictet, at the temperature of freezing, the metres, which at that temperature are equal to each other and to the ten millionth part of the quarter of the meridian, are equal to 39·38272 inches meaſured on the Engliſh rule of C. Pictet.

"Done at the Mathematical and Phyſical Claſs of the National Inſtitute, Nivoſe 6, (year 10).

"(Signed) LEGENDRE, MECHAIN; and PRONY, Reporter.

"The Claſs approves the report, and adopts the conclusions.

"Certified agreeably to the original.

"Paris, Nivoſe 26, year 10."

XL. *Letter from Count* MOROZZO *to* C. LACEPEDE, *reſpecting a Parrot hatched at Rome; together with ſome Obſervations on the duration of the Life of theſe Birds* *.

NO one can know better than you do, that it is impoſſible to have the natural hiſtory of birds ſo complete as that of quadrupeds. To render it more perfect, we muſt collect, as you have done, all the new facts and correct obſervations made ſince the immortal Buffon publiſhed his work, in order that we may form a new one ſtill more complete. That I may contribute towards this branch of natural hiſtory, permit me to ſend you an account of an intereſting fact, viz. the hiſtory of two parrots of the family of the Amazons, which laid eggs ſeveral times, and which have reared a young one, brought forth at Rome in the courſe of the preſent year 1801.

1ſt, I ſhall firſt give you a hiſtory of theſe parrots, with an account of their laying and incubation.

2d, Of their habits and attachment, and in what manner they reared the young one.

3d, I ſhall examine the ſpecies to which they belong: I ſhall give a deſcription of the male, the female, and the young parrot, and compare them with thoſe given by ornithologiſts.

4th, I ſhall relate ſome very rare caſes of parrots hatched in our climates, and the difficulties that oppoſe this circumſtance.

5th, In the laſt place, I ſhall offer ſome conjectures on the duration of the life of parrots compared with that of other birds, which will give me an opportunity of obſerving that

* From the *Journal de Phyſique*, Ventoſe, an. 10.

there

there is a conſtant relation between the time employed in incubation and the duration of their life.

I. *Hiſtory of the Parrots, their Laying and Incubation.*

The parrots of which I am going to give the hiſtory, belong to M. Paſſeri, a Roman.

In the firſt place, I muſt expreſs my gratitude to the owner for having permitted me to examine and obſerve them very cloſely, and for having communicated to me the obſervations which either he or his mother had an opportunity of making, for ſixteen years, on the manners and habits of theſe birds, which enables me to give a correct hiſtory of them.

M. Paſſeri obtained at Marſeilles, in 1786, a female parrot of the family of the Amazons. Some months after, he got, at Avignon, a male under the name of the Amazon parrot of Brazil. He put them together, being both tame, and ſuffered them to go about the houſe at their pleaſure. They never had a ſmall chain on their legs, nor any other badge of ſlavery. They often remained on the ſtick placed for them, and ſometimes they retired in the night to a large iron cage which was not ſhut; and in every other place where they have ſince been, they have always enjoyed the fulleſt liberty. From the moment they ſaw each other, they conceived a ſtrong mutual attachment, the cauſe of which, no doubt, was their difference of ſex; and their friendſhip ſtill continues to ſuch a degree, that if ſeparated for a few moments they are in the greateſt agitation, emit loud cries, and their uneaſineſs continues till they are again brought together. When they came into the poſſeſſion of M. Paſſeri they had attained to their full growth, but he had no knowledge of their age.

The male pronounced diſtinctly ſeveral French words, as it does ſtill; but the female emits only loud cries, and chatters very much, without articulating a ſingle word. They have travelled a great deal with their maſter, being firſt carried to Rome, thence to Forti, Valentano, and Nagni, and again back to Rome: during theſe journeys they were ſhut up ſeparately in ſmall wooden boxes called *ſabots**.

The female laid eggs ſeveral times. The firſt time this circumſtance was obſerved was at Forti, about five years ago. She produced two eggs, which ſhe depoſited in a hole near the kitchen chimney; but the inceſſant noiſe of the people going backwards and forwards, prevented her from continuing to hatch them. Beſides, unforeſeen circumſtances obliged the owner to change his reſidence.

* This method of carrying birds is very convenient; for, as they cannot turn themſelves, they do not ſpoil their plumage.

The

The ſecond time was at Valentano, where ſhe produced two eggs, which ſhe depoſited in a corner of the room, without preparing any neſt. Having ſat on them ſome days, it was then concluded that it would be better to place them under a pigeon; but, though the pigeon ſat on them for a conſiderable time, they were not hatched.

The female laid for the third time laſt year (1800). She produced two eggs about the middle of May, and depoſited them on the ground; but ſome days after they were found broken. Whether it was done by the male that he might enjoy the female again, or whether it happened by ſome other cauſe, is not known. In the beginning of June, however, ſhe again laid two eggs *; but ſhe depoſited them this time in an earthen veſſel which, the handle of it being broken, was employed as a chaffing-diſh: it was half filled with aſhes, and ſtood behind a door which concealed the female while ſitting.

The female ſat for forty days, and on the 25th of July one of the eggs was hatched; but the young one died next morning.

As M. Paſſeri was deſirous to confirm the fact of a parrot being hatched at Rome, he carried it to the hoſpital of San-Spirito; but putrefaction being too far advanced, it was thrown away. It was ſeen, however, by ſeveral of the young ſurgeons at that inſtitution.

The fourth, or rather the fifth time, that the female laid, (for ſhe had laid once beſides the three times above mentioned,) was in the preſent year 1801. She depoſited three eggs in the ſame veſſel filled with aſhes, which ſtood in the ſame place as before. The incubation laſted forty days, and on the 24th of June a young one was hatched: ſome days after, the other eggs were thrown away as unfruitful. This young parrot is at preſent in good health, and as large as a pigeon.

It remained almoſt naked for the firſt fortnight, being covered only with ſoft down: the ſmall gray tubes of the quills then began to appear, and from theſe iſſued, like a ſmall bruſh, the green feathers, which grew progreſſively to ſuch a degree, that on the 20th of Auguſt, that is to ſay, at about the end of two months, it was completely clothed.

On the 12th of July, that is, on the 18th day, the young parrot began to emit cries for the firſt time.

On the 14th, that is, on the 20th day, it began to open its eyes.

* It is very common for birds to lay a ſecond time, when their eggs have been broken or carried away.

On

On the 20th of Auguſt, when it was well covered with feathers, the mother, which had conſtantly ſlept in the neſt with its young, abandoned it, and ſlept by the male as uſual.

On the 25th of Auguſt, the young parrot ſlept out of the neſt.

II. *Habits of theſe Birds—their Attachment—Method of rearing their Young.*

It has not been obſerved that theſe parrots ſpeak more in ſummer than in winter. They did not ſeem to ſuffer any inconvenience from the heat: yet they waſh themſelves in ſummer, and, when the ſun is ardent, place themſelves in the ſhade: on the other hand, they ſeek for the ſun in winter. They ſeem much affected by a change of weather; and during ſtorms they are under great agitation, and ſend forth loud cries.

No particular time of their moulting has been remarked: it takes place gradually in the courſe of the year.

Theſe birds copulate only once a year, in the ſpring: it was always in the latter end of March and beginning of April that they ſhowed ſigns of it. At this time, the male redoubles his careſſes to the female, which returns them by marks of the greateſt ſenſibility. They kiſs with their bills, ſcratch each other, and ſtroke, one by one, the ſmall feathers of the head. Sometimes the male, preſſing his throat, by a ſort of rumination throws up ſome morſels he has ſwallowed, and preſents them with his bill to his companion, which eats them with great avidity.

After theſe preludes, they copulate like pigeons. They copulate ſeveral times a day: they have been ſeen to copulate even three times in a morning, and it is to be preſumed that they did ſo ſeveral times more in the courſe of the day; which proves that they are exceedingly warm.

According to our obſervations they conſtruct no neſt. They collected no feathers, bits of ſtraw, or any other light bodies within their reach. The female depoſited her eggs in the ſame veſſel, without carrying any thing to it.

The eggs are as large as thoſe of a pigeon, and are entirely white. As ſome authors have obſerved that the eggs of the Amazon parrot are ſpotted with gray, while thoſe of the parrot in queſtion were white, there is reaſon to conclude that the ſtate of domeſticity has an influence on the colour of the eggs, as it has ſometimes on that of the plumage. This obſervation did not eſcape Ariſtotle *.

The number of eggs was always two, except once, when

* Hiſt. Animal. lib. vi. c. 2.

there

there were three. Though theſe eggs were often handled, the female never made the leaſt heſitation to ſit upon them *.

Of the eggs laid this and the preceding year, only one was hatched.

The female alone ſits on the eggs. The period of incubation is forty days †. The female ſcarcely ever leaves them, except to drink, or for any other neceſſary purpoſe; and then ſhe immediately returns. During that time, the male ſtands ſentinel at a little diſtance: he is very attentive to the leaſt noiſe, and never ſtirs from the ſpot but to carry food to the female, which he always breaks with his bill during the incubation.

When the young parrot is hatched, both the old ones carry it food. The young one did not begin to eat alone till the 70th day. The old ones continued to cut the food with their beaks for more than ten days.

They gave to the young one the ſame food as they uſed themſelves. Being accuſtomed to every thing, they ate rice, meat, paſte, Turkey corn, hemp-ſeed, and fruits; but they were fondeſt of the ſeeds of the turnſole, carthamus, melon, and cucumber.

The following fact is highly worthy of attention:—The owner ſeeing that his young parrot increaſed in ſize, and fearing that the veſſel would be too ſmall to contain the mother and her young one, he put into a baſket ſome linen rags and feathers, depoſited it in the ſame place where the veſſel ſtood, and conveyed the young parrot into it: the mother immediately repaired to the baſket, and ſeemed highly ſatisfied with her new habitation; but ſome hours after, ſhe began to cut with her beak one edge of the baſket: in three days ſhe finiſhed her work, having made an aperture in it four or five inches in width and ſix or ſeven in height. The twigs were cut as neatly as if by the ſharpeſt inſtrument.—It appears to me that this fact can be explained only in two ways. The mother, ſeeing her young one grow big, cut the edge of the baſket, that when it was in a ſtate to go abroad it might have no occaſion to climb over the edge, which was about ſix inches in height.

III. *Deſcription of the Male, the Female, and young Parrot, in order to aſcertain to what Species they belong.*

Had ornithologiſts left us exact deſcriptions, it would be eaſy to compare different ſpecies: theſe deſcriptions are very often deficient and imperfect, and very often the male alone,

* Buffon made the ſame obſervation, vol. ix. 8vo. p. 287.

† Buffon ſays that the male and female Amazon parrots ſit alternately.

or the female, has been delineated. It is well known that there is a difference among birds according to their ſex; and this has produced great ambiguity, of which many inſtances might be produced: the caſe has been the ſame, in my opinion, with the male in queſtion.

The male is as large as a pullet of four months old: its length from the root of the bill to the extremity of the tail is 14 inches. The top of its head is of a beautiful yellow colour, which extends to the throat and the lower part of the neck. On the head, the feathers appear to have an orange tint, becauſe there is a double row, the longeſt of which are yellow, and the ſhorteſt of a roſe colour. The reſt of the neck, the upper part of the back, and the cover of the wings, are of a brilliant green. The breaſt and belly are of a yellowiſh green, and the tip of the wings of a beautiful yellow intermixed with orange. The plumes of the wings are variegated with blue, violet, and very bright red. The two exterior feathers on each ſide of the tail have their internal barbs red at the origin of the quill, and of a dark green at the extremity, which is yellowiſh green: the other feathers are of a dark green, terminating with a yellowiſh green. On the thighs it has very thick yellow feathers, like breeches *à la Suiſſe:* the bill is of an aſh-gray colour, black at the tip: the iris of the eye is orange: the feet are gray, and the claws black.

According to this deſcription it appears that, by the head and tips of the wings, it may belong to the family of the criks; but its ſize, and the plumage of the back and belly, ſeem to give it a reſemblance to the firſt ſpecies of the yellow-headed Amazon of Buffon. The large yellow breeches *à la Suiſſe*, as far as I know, have been deſcribed by no ornithologiſt; and this peculiarity is ſufficiently remarkable to make a ſeparate claſs: beſides, the iris of the eyes of the parrots in queſtion is orange inſtead of yellow, as in the criks; and the bill is aſh-coloured; while that of the Amazons is red at the bottom, and that of the criks white.

The female, which is ſmaller than the male, is as large as a pigeon: her length from the root of the bill is only $11\frac{1}{2}$ inches. The top of the head is yellow, and the tips of the wings of a bright red. The plumage has a perfect reſemblance to that of the yellow-headed Amazon of Buffon: it is certainly the *Pſittacus major, viridis alarum, coſta ſuperne rubente*, the Amazon parrot of Barrere *, and not the *Pſittacus macrourus viridis, genis nudis, humeris coccineis; Pſit-*

* France Equinoxiale, p. 144.

tacus

tacus nobilis of Linnæus *. In my opinion Linnæus alluded to ſome ſpecies of Ara, as the individuals in queſtion have not their cheeks bare. There can be no doubt that the female is the real Amazon, but it is difficult to determine to what ſpecies the male belongs. It is poſſible that the latter may be the male of this ſpecies, and that ornithologiſts may have examined and deſcribed only the female, and may have ſuppoſed both to be perfectly ſimilar; which, however, is rarely the caſe. It cannot be claſſed either with the ſpecies approaching the yellow-headed Amazon, and known under the name of the baſtard Amazon, or demi-Amazon; for none of theſe have yellow breeches on the thighs: to obtain, therefore, a complete hiſtory of theſe birds, it would be neceſſary to deſcribe always both the male and the female.

The young parrot, when examined at the third month, had the head and lower part of the neck yellow, like the father: its length from the root of the bill was about 10 inches, and, at four months, 11 inches. The body is exactly like that of the father; but its plumage is of a much livelier and more ſplendid green. The tips of the wings are orange. the ſmall red feathers being interſperſed with yellow: it has the yellow breeches, but leſs tufted than thoſe of the father; the ſame legs, gray feet, and black claws: the bill is gray, and blackiſh at the point. The eye till the third month was gray, and the iris of the ſame colour; but it gradually changed, and at the fourth month it was yellow. I have no doubt that it will become orange, like that of the father. Its tongue is very black, and the noſtrils well marked: the progreſs it has made in ſize during the courſe of four months, induces me to think that it will ſtill grow, and become larger than the father.

This living individual may afford an opportunity of making many important obſervations, which it would be intereſting to follow.

1ſt, To know at what age it will attain to its full growth.

2d, Whether any change will take place in its plumage.

3d, At what age it will be in a ſtate to procreate.

4th, How many years it will live.

I have requeſted the owner of this parrot to attend to the three firſt articles; for, in regard to the duration of life, as I believe it to be very long, it can be known only to the ſecond or third generation after us. I have begged him alſo to note down to what age the mother will lay. At preſent ſhe muſt be about twenty years of age, for it is about fifteen

* Linn. Hiſt. Nat. ed. 10. p. 97., ed. 13. p. 140.

ſince ſhe came into his poſſeſſion; and, as ſhe was then of the full ſize, ſhe muſt at leaſt have been five years of age. I begged him likewiſe to be attentive during the next year, and to obſerve whether one egg alone will produce, and whether the reſt will be equally fruitful.

IV. *Inſtances of ſome Parrots which have laid in Europe.*

The parrots both of the old and new continent never paſs the tropics, and ſeem confined to a zone of 23 degrees on each ſide of the equator: in their wild ſtate they never paſs theſe limits, which nature ſeems to have preſcribed to them. When tranſported beyond theſe latitudes, they live, and are ſenſible to love, notwithſtanding the difference of climate. We have ſome inſtances therefore, but very rare, of parrots producing young in our temperate countries; though it often happens that they lay eggs without any germ. Buffon aſſerts, on the teſtimony of the *Gazette de Litterature* of the 17th of November 1774, that M. de la Pigionière had a male and a female parrot in the town of Marmande, in Angumois, which for five or ſix years never failed to lay eggs that produced young, which were reared by the father and the mother. Theſe parrots were of the ſpecies called the *jaco*, or aſh-coloured parrot of Guinea, *Pſittacus cinereus ſeu ſubcæruleus* of Aldrovandi. He quotes father Labat, who ſpeaks of two parrots which ſeveral times produced young at Paris; and the abbeſs of Beaumont-les-Tours, who had two young parrots hatched in the month of January; but the cold ſoon killed them.

Theſe are ſingle facts which give us no information reſpecting the habits of theſe birds either during the time of their laying or incubation. We, however, no where find that the Amazon parrots, or any others of America, ever produced in Europe.

Though the climate of Rome is not ſo cold as that of Paris, as the mean temperature at the end of March and the commencement of April is 17 degrees of Reaumur *, I am not of opinion that this was the principal cauſe of theſe parrots producing; as the ſame thing muſt have often taken place in Sicily, Malta, and other countries lying more to the ſouth: but of this we have no inſtance. I am therefore induced to think that the parrots in queſtion produced at Rome, becauſe they had been long together, were exceedingly fond of each other, when they became acquainted, and always enjoyed the completeſt liberty; becauſe they were probably of the ſame age,

* Tab. Meteorolog. de Rome, par l'abbé Scarpellini.

and

and becauſe the owner, finding that the eggs were not clear like thoſe of the preceding year, but contained a germ, thought proper to leave them in a detached chamber expoſed to the ſun, gave the parrots more liberty and abundance of food. I am fully perſuaded that theſe cauſes contributed much more than the climate to their producing.

If inſtead of one young one, which I believe to be a male, there had been a female alſo, it is very probable that a breed might have been eſtabliſhed at Rome. We may hope for it the next year.

Conjectures reſpecting the Duration of the Life of Parrots, deduced from the Time of their Incubation, and compared with that of other Birds.

Salerne aſſerts that he ſaw a parrot 60 years of age ſtill alive and active; M. de la Borde ſays that he ſaw one which was more than 46 years, and I myſelf have ſeen one which had been in the ſame family 75 years.

Though Ollina, an excellent obſerver, was of opinion that the life of parrots, taken at a medium, did not exceed twenty years, I am of opinion that he was deceived, having, perhaps, had an opportunity of obſerving only a ſmall number of individuals.

All naturaliſts agree in aſſigning a long life to parrots, to which Linnæus gives the epithet of *longævi*. This fact, then, is inconteſtable. The parrots in queſtion furniſh a new proof of it. According to the calculation already made, the age of the female cannot be leſs than 20 years; for, as it is five ſince ſhe firſt laid, it thence reſults that ſhe was then 15; but if in the order of nature theſe birds begin to lay at that age, this would be another proof of their longevity, ſince we know that the large birds, with which we are beſt acquainted, are capable of laying before they are a year old, and that they live at leaſt nine or ten times as long: we may thence conclude that theſe parrots muſt live 130 or 150 years. It may be objected, indeed, that it ſometimes happens that the females do not become mothers till an age greater than that eſtabliſhed by nature: but this is rarely the caſe among quadrupeds, and ſtill leſs ſo among birds. The parrots in queſtion lived together ten years without laying, though they enjoyed the ſame liberty and the ſame facility of copulating as they afterwards had; and therefore we may ſuppoſe that nature was not diſpoſed for it.

Buffon thinks that the life of birds is longer in proportion than that of quadrupeds, according to the time employed in their growth. As we can obtain certain data reſpecting the

time employed for the whole growth only in regard to a ſmall number of individuals, let us try whether we cannot, by induction, diſcover that the longevity of birds is proportioned to the duration of their incubation.

I ſubjoin the following table in order that the reader may ſee, at one view, the period of the incubation of different birds, and the duration of their life in the next column. I muſt however obſerve that the time of incubation is not altogether the ſame in all climates: in warm climates it is accelerated, and in cold climates retarded. But the difference is very ſmall, and can occaſion no ſenſible difference in the table.

Table of the Period of Incubation, and of the Duration of Life of ſeveral Birds.

Names of the Birds.	Incubation. Days.	Duration of Life. Years.	Authors.
The ſwan	42	200 nearly	Aldrovandi.
Parrot	40	100 nearly	Wolmaer.
Gooſe	30	80 and more	Willughby.
Eagle	30	Never obſerved by any naturaliſt.	
Buſtard	30		
Duck	30		
Turkey	30		
Peacock	26 to 27	25 to 28	Ariſtotle. Pliny.
Pheaſant	20 to 25	18 to 20	Treatiſe on Pheaſants.
Crow	20	100 and more	Heſiod.
Nightingale	19 to 20	17 to 18	Buffon.
Pullet	18 to 19	16 to 18	Buffon.
Pigeon	17 to 18	16 to 17	Several obſervat.
Linnet	14	13 to 14	Willughby.
Canary bird	13 to 14	13 to 14	Treatiſe on theſe Birds.
Goldfinch	13 to 14	18 to 20	Buffon.

We ſhall here obſerve, that it appears by this table that the ſwan and parrot, which employ the longeſt time in incubation, are not thoſe which live longeſt: the longevity of the former of theſe birds has been carried to 200 years, and that of the latter to 100.

In regard to the oſtrich, the life of which the antients carried beyond 100 years, we can find no relation between this period and that of the incubation, as the female ſeldom ſits on the eggs, which are hatched by the heat of the ſun. The caſe is the ſame with the crane and the heron, to which the

the antients aſſigned a life equally long; but we have no obſervations in regard to their incubation *.

The gooſe, which employs thirty days in incubation, lives beyond the age of 80. The eagle, buſtard, turkey, and duck, are the next in the table in regard to the period of incubation; but it has never yet been compared by any one with the duration of their life.

All the other birds mentioned in the table, live nearly in proportion to the time of their incubation; but, as theſe obſervations were made chiefly on domeſtic or captive birds, it is probable that there may be ſome difference between theſe and thoſe which are in the wild ſtate.

"We know beſides," ſays M. de Buffon, "that we never give time to our domeſtic birds to go through the complete period of life aſſigned to them by nature."

In regard to birds in their wild ſtate, it is almoſt impoſſible to obtain any correct information. Very few ornithologiſts, therefore, have ſpoken of the duration of their life; and when they are caught in ſnares, they endeavour to judge of their age by marks which are very ambiguous. (See *Ariſtotle.*)

The conſtant ratio which I have obſerved between the time of incubation and the duration of the life of birds, ſuggeſted to me the idea of examining what relation there is between the duration of the life of quadrupeds and the time employed in geſtation; and I have made out the following table:

Table of the Period of Geſtation and of the Duration of Life of ſeveral Quadrupeds.

Names of the Animals.	Geſtation. Months.	Durat. of Life. Years.	Authors.
Elephant	24	150 to 200	Ariſtotle.
Camel	12	40 to 50	The ſame.
Buffalo	12	20 to 25	Cajetano.
Aſs	12	25 to 30	Buffon.
Mare	11½	25 to 30	Ariſtotle.
Cow	9	14 to 15	The ſame.
Stag	8½	35 to 40	New Treatiſe on Hunting.
Roebuck	5½	12 to 15	Buffon.
Sheep	5	12 to 15	The ſame.
Goat	5	11 to 13	The ſame.
Dog	2	11 to 15	The ſame.

By comparing theſe two tables a very great analogy will be found between the time of the geſtation of quadrupeds

* See Paulus Jovius.

and that of the incubation of birds, in regard to the duration of life. Thus we obſerve that, among quadrupeds, the elephant, which lives from 150 to 200 years, goes with young 24 months; and that the camel, which goes with young 12 months, lives 40 or 50 years. Among birds, the ſwan, which ſits 42 days, and the parrot, which ſits 40 days, are thoſe which live longeſt, their life exceeding 100 years. In looking over this table, the ſame relation appears to be preſerved. The horſe and aſs, which have the ſame period of geſtation, live nearly the ſame time; that is to ſay, 26 or 30 years. The domeſtic fowl and pigeon, which have nearly the ſame period of incubation, attain to the ſame age; and the caſe is the ſame with the goat and the ſheep, as well as with the canary-bird and the linnet.

I muſt, however, confeſs, that in theſe tables there are two very ſtriking exceptions. The ſtag, among quadrupeds, goes with young a month leſs than the mare, and yet lives about ten years longer: and, among the birds, I obſerve that the crow, which ſits only twenty days, lives far beyond 100 years, according to Heſiod and ſeveral other obſervers.

Theſe exceptions ſtill further prove that our knowledge reſpecting this part of natural hiſtory is very limited, and prove the neceſſity of making more obſervations. Beſides, the difficulty of aſcertaining the exact duration of the life of quadrupeds is equally great as that experienced in aſcertaining the duration of the life of birds, ſince theſe obſervations can be made only on a ſmall number of individuals, taken, for the moſt part, in a ſtate of domeſticity; and M. de Buffon ſagely remarks, that man, wiſhing for continual enjoyment, ſhortens the period.

If, notwithſtanding theſe exceptions, ſome relation has been ſuppoſed to be found between the time employed for perfect growth, and the duration of life, it appears, from a view of this table, that there is a more direct connection between it and the time of geſtation: nature alſo ſeems to have preſerved a kind of connection between the time employed in geſtation and that of incubation in regard to the duration of the life of birds and quadrupeds.

The period of the incubation of the parrot, which no naturaliſt had before obſerved, has led me into conſiderations ſomewhat prolix; but ideas ſuggeſted merely as hints, when aided by experience and more correct obſervation, often conduct to important truths.

XLI. *De-*

XLI. *Description and Natural History of the Wild Goat of the Alps.* *By* M. Berthout Van Berchem *jun.*

[Concluded from p. 121.]

But having ſaid enough in regard to this animal in its confined ſtate, let us now haſten to reſtore it to liberty: it is on the ſummits of the ſteepeſt rocks, ſuſpended, as we may ſay, between the heavens and the earth, that it ought to be conſidered, as it is there that it diſplays all its ſtrength and agility. It is particularly intereſting to ſee the eaſe and facility with which it traverſes the moſt awful rocks, the bare ſight of which inſpires terror, and which are inacceſſible to man. When it leaps, it does not ſeem to take a ſpring; it directs its ſight with the greateſt preciſion to the place it wiſhes to reach, and never miſſes its aim. When tranquil, it generally carries its head low; but when running, it holds it elevated: it even throws it ſomewhat back, which heightens the agreeableneſs of its figure. When it wiſhes to raiſe itſelf to the top of a perpendicular rock fifteen feet in height, it does it at three leaps, or rather three ſucceſſive jumps, of five feet each: one might ſay that it ſcarcely found any point of ſupport on the rock; and it appears to have touched it only that it might be thrown higher, with the ſame force that an elaſtic ball would be if impinged againſt a hard body. It never makes more than three leaps in this manner. If it be between two rocks ſtanding near each other, and wiſhes to get to the top, it accompliſhes its object by jumping from the one to the other. It traverſes the glaciers alſo with great rapidity, but only when chaced: when its courſe is voluntary it avoids them, and does not ſeem to walk on them with ſuch confidence as among the rocks.

I ſhall now give an account of the manner in which the wild goat lives when peaceably ſuffered to follow its natural habits, when not moleſted in its retreats; and ſhall then deſcribe the manner in which it is hunted. We ſhall ſee in what manner man, who of all beings is the moſt deſtructive, penetrates into the almoſt inacceſſible places which nature has aſſigned to this animal, and where, as appears, it ought to be ſheltered from his rapacity. We ſhall ſee how, at the riſk of his life, and braving the moſt imminent dangers, he purſues it, and has diminiſhed the number in ſuch a manner, that, though it was formerly diſperſed over all the high mountains of Swiſſerland, Savoy, and the Valais, it is now found only in ſome places, and even in ſmall numbers.

Theſe goats feed during the night in the higheſt woods, and never further than a quarter of a league from the ſummit; but, when the ſun begins to illuminate the tops of the mountains, they leave the woody regions in flocks, aſcend the mountains as they feed, and proceed ſometimes to very conſiderable heights. They ſeek the declivities turned towards the eaſt and ſouth, and lie down in the higheſt and moſt elevated places. But when the ſun has got through three-fourths of his courſe they deſcend to the woods, feeding as they proceed, in order to paſs the night. When it threatens to ſnow, they deſcend alſo into the woods for the ſake of ſhelter.

Theſe animals aſſemble in flocks of 10, 12, or 15; formerly they were more numerous, but at preſent they are often in ſmall numbers. All the males ſix years of age, and thoſe above it, always remain in higher places than the females or the males that are younger. The older they are, they are the leſs fond of living in ſociety. They become hardened againſt the cold, froſt, and all the ſeverity of the ſeaſons. Very often theſe old goats have the tips of their ears withered, and, as it were, dead. Severe cold muſt neceſſarily produce this effect on extremities where it is well known the circulation of the blood is eaſily checked. They lead a ſolitary life on the ſteep ſummits, and never deſcend from them. To conclude what we have to ſay in regard to their habits, we ſhall here obſerve, that in winter they are accuſtomed to place themſelves on eminences oppoſite to that part of the horizon from which the ſtorm proceeds, and to remain there like ſtatues: they only go from time to time to feed in the neighbourhood, and then return to their ſtation. The females and young have not this habit; they remain at all ſeaſons in places leſs elevated.

As the fore-legs of theſe animals are ſhorter than thoſe behind, it is natural to ſuppoſe that they can aſcend with more facility than they deſcend. Nothing but the ſeverity of the weather can induce them to remain in the low diſtricts; and, if a few fine days take place in winter, they immediately quit the woods and aſcend the mountains.

Winter is their ſeaſon of love, and the month of January that of their greateſt heat. All the goats, of whatever age, then mix together; the males combat, and the females belong to the victors; after which they return to their uſual order. The females go with young five months, and bring forth in the laſt week of June or firſt of July. They produce only one at a time, of the ſize of a cat, and which, like the young chamois, walks as ſoon as dropped. An hour after its

its birth it can conceal itſelf among the rocks *. When the female is delivered ſhe licks its eyes, head, and neck; teaches it, at an early period, to leap; and beſtows on it the care of an attentive and vigilant mother. While ſuckling, ſhe remains in the holes of the rocks. She calls her young by bleating; but the hunters think they have remarked that it is by geſtures that ſhe uſually expreſſes her deſires.

As it often happens that flocks of wild goats, chamois, domeſtic goats, and ſheep, paſture at a ſmall diſtance from each other, I endeavoured to find whether theſe animals ſometimes intermixed and produced in that ſtate of liberty; but I could obtain no certain information reſpecting this fact, which would be of great importance to natural hiſtory. It only appears that the wild goat and the chamois never intermix in their natural ſtate.

The beſt time for hunting the wild goats is towards the end of ſummer, and in autumn, in the month of Auguſt and September. It is then that they are in beſt condition. This ſpecies of hunting is exceedingly dangerous and fatiguing; and therefore can be undertaken only by the inhabitants of the mountains. The hunters, beſides reſolution to look down without fear from the greateſt heights, muſt have ſufficient addreſs and ſteadineſs of foot to get out of difficult paſſes, and to take accurate aim. They muſt alſo be endowed with great ſtrength and vigour to endure hunger, cold, and the greateſt fatigue. The moſt determined hunters reſide in the mountains of the Lower Valais: almoſt all the peaſants of Servan † follow

* A hunter one day ſurpriſed a female chamois while bringing forth her young one on a rock: a ſentiment placed by nature in the breaſt of all mankind retained his arm, and prevented him from killing her: he viewed this intereſting ſpectacle for ſome moments, and ſaw the mother lick her kid. He then prepared to fire, but the chamois and her young one both inſtantly diſappeared, and concealed themſelves among the rocks.

† The village of Servan is ſituated in the mountains of the Lower Valais, and at the diſtance of two or three leagues from Martigni: its ſituation is the wildeſt and moſt picturesque I ever ſaw It ſtands on the declivity of a chain of mountains which borders on the weſt that part of the valley of Trient, which is only a continuation of that of Orſine, in Upper Faucigny. At the bottom of this valley runs the water of Berard with a roaring noiſe, and, uniting itſelf to the Trient, aſſumes its name. Torrents, which daſh down on every ſide from the ſummits of the mountains in ſuperb caſcades, contribute to enlarge its ſtream. That ſilence of nature, that calmneſs, at the ſame time awful and delightful, which is experienced in the middle of a foreſt during a fine ſummer's night, never prevails in this abode. The air is continually agitated by the noiſe of the torrent which flows through the middle of the valley, and by that of the numerous caſcades which join it. But this continual and hollow roaring of the water is often increaſed by the tumbling down of large maſſes, and the fall of

follow this occupation; and as the animal is not found in their mountains, they go to hunt in the Eaſt Valley, having firſt obtained permiſſion from the inhabitants for that purpoſe. A hunter never goes out alone; he generally aſſociates with himſelf one or two others, furniſhed with rifle guns* and a bag of proviſions.

They aſſociate for the purpoſe of gain, but dangers and perils unite them; and indeed there are many reaſons why two hunters ſhould be united. Being obliged to ſpend the night among the rocks, and at very great heights, they conſtruct a wretched hut with pieces of turf, where they ſleep without fire or bedclothes, and on their awaking often find the entrance of it blocked up by two or three feet of ſnow. When ſurpriſed by night while in the purſuit of a wild goat, it frequently happens that, not being able to diſengage themſelves from the rocks, they are obliged to paſs the night ſtanding in

of the rocks, which, attacked by the ſlow action of the moiſt elements, crumble to pieces through age, and choke up the bottom of the valley. Theſe torrents, by tearing open the ſides of the mountains, unveil to us their interior ſtructure. In this country, every thing preſents the image of deſtruction: it is in the middle of theſe degraded foreſts of firs and pines, of thoſe accumulated rocks which paſs through all the ſtates of decompoſition to ſand, and from the ſtate of ſand to that of mould, that the geologue diſcovers the compoſition of mountains, and collects facts for the theory of the earth. But in the midſt of this diſorder one may diſcover the hand of man and the labour of induſtry: excited by his wants; ſheltered from all quarters by the rocks, and in a good expoſure, the inhabitants have collected a little earth, which they cultivate and ſow with corn to ſupply them with food.

Of all the caſcades with which this valley is decorated, the moſt intereſting for the naturaliſt and the painter is that, no doubt, of Maupas, between the village of Finio and that of Servan. The torrent of the Trieve, the white foaming waters of which daſhing againſt the rocks, which they break and carry with them to their bed, form ſeveral caſcades; between very high rocks, a plank laid acroſs ſerves as a paſſage, and from this tremendous bridge the torrent is ſeen precipitating itſelf from one fall to another, and flying to a diſtance below the feet; before is a rock cut into the form of a peak, and which ſeems to be an inſurmountable barrier; but a ſmall narrow path, cut out in a zig-zag form and furniſhed with a parapet, enables the traveller to paſs over it, and to conſider this ſublime and magnificent view, which the horrid noiſe of the torrent renders ſtill more ſtriking. The more you advance towards Servan, the leſs ſteep the declivity of the mountain becomes, while the valley becomes broader and better cultivated. But in traverſing this wild country one is not aſtoniſhed that the inhabitants ſhould ſeek for and be fond of thoſe dangers to which they expoſe themſelves in hunting the chamois and wild goats: they are bred up to this occupation; the rocks and precipices are, as we may ſay, their element, and when they quit their habitations they find no perils to which they are not accuſtomed.

* Theſe pieces can fire twice without being reloaded; but they have only one barrel, with two locks. See a deſcription of them in *De Luc's Recherches ſur l'Atmoſphere.*

each

each other's arms, that they may remain awake: theſe are ſituations which none can have an idea of but thoſe who have traverſed the rocks. I cannot here help making one reflection: It is among the inhabitants of theſe wild and horrid mountains that I found the greateſt honeſty and friendſhip; in a word, all thoſe virtues which render men worthy of eſteem.

As theſe animals aſcend to the higheſt regions early in the morning, the hunters muſt get to thoſe places before them. The goats are then ſeen coming to paſture, and, though their ſmell is exceedingly acute, they do not ſmell the hunters, who, concealing themſelves behind the rocks, ſuffer them to approach within 30 or 40 paces before they fire.

The following appear to be the moſt natural cauſes why theſe animals do not ſmell the hunters:—In the morning the ſun heats and rarefies the upper regions of the air, while the lower ones are ſtill cold and condenſed. From this want of equilibrium there muſt neceſſarily reſult a current which proceeds upwards, and which conſequently carries in that direction the emanations proceeding from the hunter, who is above the goats. This effect, indeed, muſt take place during the greater part of the day; for the ſun, by warming the declivities of the mountains, muſt produce along them an aſcending current. Beſides, as theſe animals feed as they go along, their noſtrils become filled with dew, which muſt have a very great influence on their ſmell. However, it is eſſentially neceſſary to get to the heights before the goats reach them; if they anticipate the hunter, they ſmell him, and make their eſcape: it would in that caſe be of no uſe to follow them; for, when once put to flight, they never ſtop till they get to a very great diſtance, and when they think they are beyond all danger *. It is ſingular that the animal never flies till it ſmells the hunter; for if it only ſees him it

* The reader will not be diſpleaſed to ſee here an account of the manner in which the inhabitants of the iſland of Crete hunt the wild goat, as deſcribed by Belon:—Some of the peaſants who live on the ſummits of the high mountains are ſuch dexterous ſhooters with the bow, and principally around the mountain of Spachia and Madara, that they pierce them with their arrows at the diſtance of 25 paces; and, to be more ſure of them, carry females, which they have bred up and tamed from their youth, and tie them up in ſome paſſage of the mountains where the males are accuſtomed to paſs. The hunter conceals himſelf behind the buſhes oppoſite to the wind, knowing that the male goats have ſo acute ſmell, that they could ſmell them at the diſtance of 100 paces. The male finding the female in his way ſtops, and the peaſant then diſcharges his arrow.—*Obſerv. fol.* 14. *verſ.*

is ſatisfied with whiſtling and looking at him, but as ſoon as it ſmells him it takes to flight.

The female never abandons her young, unleſs ſhe is hunted: if it cannot follow her ſhe leaves it, and the latter conceals itſelf in the burrows of the marmot, or other holes of the rocks; ſometimes at the depth of ſix feet.

When the danger is over ſhe returns to call it, and ſearches till ſhe finds it: but if ſhe remains too long, the young goes to look for the mother; it iſſues from its hole, calls its mother, and enters ſome other hole with fear and emotion, and with ſigns of the greateſt uneaſineſs. If it ſees the mother it runs up to her; but if ſhe is wounded, and lying, as ſoon as it ſmells the blood it betakes itſelf to flight; then returns, approaches with the ſame eagerneſs, and again retires for the ſame reaſon. It can hardly be conſoled for the loſs of its mother; ſearches for her a long time, and never quits the place where it was brought forth and where it loſt her.

When a wild goat is wounded, all the reſt make their eſcape and abandon it: they are ſeized with fear and terror, and fly with the utmoſt rapidity. The hunter can diſtinguiſh the wild goats that have been wounded, by their walk, which is ſlow. They ſuffer their heads to fall ſometimes to the one ſide and ſometimes to the other, and they ſoon after lie down, even when the wound is ſlight.

Man, though the greateſt enemy and principal deſtroyer of theſe wild goats, is, however, not the only one whom they have to dread. The wolves and eagles purſue their young *. The mothers, on theſe occaſions, eſcape with them to caverns or hollow places in the rocks, make their young enter firſt, and then, having entered themſelves, preſent their head to the aperture in order to face the enemy.

There are few animals the natural hiſtory of which is diſfigured with more fables by ſuperſtition, and more errors by ignorance, than that of the wild goat. The naturaliſt, as already obſerved, is obliged to truſt to the hunters, and theſe indemnify themſelves for the labour and fatigue they have experienced by the falſehoods which they propagate: beſides, an animal the hunting of which is ſo painful, ought, in the eyes of the vulgar, to have ſomething of the marvellous connected with it. Hence the property aſcribed to the horns of the wild goat of deſtroying the force of poiſon; and hence the opinion of the hunters, that if their pieces are rubbed with certain herbs they cannot hit the mark. We ſhall

* See an account of the combats of the lammer-geyer and the chamois in *Cox's Letters on Switzerland*, vol. i. p. 267.

not

not enlarge on theſe popular tales, but we think it neceſſary to refute two opinions which are very much believed among naturaliſts: one is, that when the wild goat or the chamois is purſued by the hunter among the rocks, they return, attack him, and puſh him down the precipices: this is a miſtake: it ſometimes happens that, if one of theſe animals is wounded, it endeavours to eſcape when the hunter approaches it, and, as it always ruſhes towards the precipices, it overturns the hunter if he happens to be in its way; but it never ſearches for him, and, on the contrary, avoids him. It is ſaid alſo that the wild goat throws itſelf down precipices, falls on its horns, and rarely ſuſtains any injury. But I can aſſert that it always leaps from one rock to another; and that it is ſo expert, and ſo ſure of its aim, that it has never any occaſion to throw itſelf on its horns: that if this ever happens, it is by accident; and beſides, it is ſufficient to look at the animal to be convinced that it could not take ſuch leaps with impunity. It has a long body, its legs are ſhort, its limbs are thick, and it weighs nearly 200 pounds: how, then, is it poſſible that it ſhould throw itſelf from the rocks without being torn by their ſharp points, or daſhed to pieces by its fall? I have been more particular in regard to this fact, becauſe the ſame error is not only related by moſt naturaliſts, but is received alſo among the Tartars *, who are hunters of wild goats, like the people of the Valais of Swiſſerland; and becauſe this conformity of opinion among people ſo diſtant might induce ſome to believe in the reality of the fact. But is it not well known that the ſame prejudices are found among different nations? Ignorance is every where the ſame, and every where the ſame objects have given riſe to the like errors. Thoſe naturaliſts who are ſo fond of final cauſes, wiſhing to give utility to the large horns of the wild goat, have adopted this opinion; and it is thus that the mania of wiſhing to explain every thing injures the cauſe of truth, and often perpetuates popular tales. M. Pallas, deceived by the accounts of the Tartars, mentions, as a proof of this aſſertion, a wild goat which he ſaw having one of its horns broken †: but this, in all probability, was owing to another cauſe. Their large, thick, and almoſt uſeleſs horns are often broken by the tumbling down of the rocks; and on theſe occaſions they are often killed themſelves. For this reaſon, the inhabitants of Cormajor traverſe the bottoms of the mountains to collect the horns carried down by the ſnow. M. Pallas ſays, alſo, that the wild goat is of ſo ſavage a na-

* Pallas *Spicil. Zool.* faſc. xi. p. 50.

† Spicil. Zool.

ture

ture that it never can be tamed. If he alludes to the old wild goats, he is right; but the inſtances above mentioned prove that, if the wild goat be caught young, it is ſuſceptible of education: and this opinion is confirmed by that of Belon, who ſays, that in the iſland of Crete this animal is tamed when young.

When one of theſe animals has been killed, it is left on the ſpot till it becomes cold. The entrails are then taken out, and the blood is put into the ſtomach. In conſequence of this method it never acquires a bad ſmell, which takes place when this precaution is neglected. Large wild goats, without the entrails, weigh 180 or 200 pounds, of 18 ounces. The female from 70 to 80 and 90 pounds. The fleſh of this animal is excellent; it has the taſte of mutton, but is more ſucculent.

The ſkin can be dreſſed exceedingly well: it becomes very pliable, and reſembles that of the goat. It is employed for manufacturing ſhamoy leather. Various ſmall articles are made of the horns, ſuch as cups and goblets. The blood is employed in falſe pleuriſies; but, as its properties are owing to the plants on which the animal feeds, that of the common goat and ram, when fed in the ſame manner, has the ſame efficacy *.

The ſale of theſe goats is liable to many variations: it depends on the weight of the beaſt, and the eagerneſs of the purchaſer. Formerly they were cheaper, becauſe more common. At preſent they are ſold ſometimes for four louis-d'ors. The chamois alſo is ſold for one louis-d'or. The wild goat is fond of ſalt: it licks the rocks which contain Epſom or Glauber's ſalt, and the waters which hold it in ſolution. It feeds chiefly on the wild plants that grow on the higher Alps, ſuch as ſilky wormwood, *artemiſia glacialis*; but in winter it eats the lichens and young ſhoots of the trees. Like that of Siberia †, it prefers thoſe places which abound with the dwarf birch, the Alpine willow, the *thalictrum*, ſaſſafras, roſe-wort, *rhodiola*, the willow-herb, *epilobium*, &c.

Theſe animals are much ſcarcer than they were formerly; they are found at preſent only in ſome mountains of Savoy and the Valais, whereas formerly they were diſperſed throughout all the higher parts of Swiſſerland ‡. But this is not aſtoniſhing;

* Hiſt. Nat. par Buffon, vol. xii. p. 164.

† Spicil. Zool. faſc. xi. p. 49.

‡ My friend M. Wittenbach, firſt paſtor of the church of St. Eſprit at Berne, who has travelled over and deſcribed the mountains of Swiſſerland with the eyes of an enlightened naturaliſt and the ſpirit of a good obſerver,

aſtoniſhing; nature, man, the beaſts and birds of prey, are all enemies with which they are ſurrounded, which perſecute and deſtroy them, and againſt which they have no other reſource than ſpeedy flight: but this rarely preſerves them. This animal, therefore, is now confined to a ſmall number of the ſteepeſt ſummits.

It is found in the valley of Cormajor, to the ſouth of Mont-Blanc, on the ſouthern declivity of that chain of mountains, and in the diſtrict between Mont-Blanc and the frontiers of the Valais; but it is never ſeen in the chain oppoſite to that of Mont-Blanc, and which forms the other ſide of the valley of Cormajor. It exiſts alſo in the mountains which form the Val-Savaranche. But it is among the mountains of the valley of Cogne, which is the frontier of the valley of Pont, in Piedmont, that it is now moſt common, and always on the declivities expoſed to the ſouth.

It is no longer found in the Valteline, the frontier of the Valais; but it muſt ſtill exiſt in the mountains between the valley of Seſia and that of Vieche. It was hunted, about 30 or 40 years ago, in the mountains of Upper Faucigny: but the breed is now entirely deſtroyed in that quarter; and it is probable, if the hunters continue to purſue it with the ſame ardour, that the race will become totally extinct.

In reflecting on the fury with which theſe animals are purſued and deſtroyed, the cauſe of it appears to me to have been the great abundance which there was of them formerly; and, though a ſingle goat did not produce much, as a great many were killed during one hunting excurſion, the gain was conſiderable. At preſent, ſince the number has greatly decreaſed, and ſince the hunters often go a great way without finding any, their ardour ought to be relaxed in conſequence of the dangers to which they are expoſed: and beſides, the inhumanity of perſecuting and tormenting a mild and timid animal, which has no other reſources to defend itſelf but flight and

ſerver, has furniſhed me with the following notice:—"There were formerly wild goats in our Alps, but none of them are now to be found. I have ſeen at Unterſeven two ſuperb horns of that animal, which is ſaid to have been killed formerly in the Alps of Lauterbrunn. Theſe animals were deſtroyed by the hunters, as well as the caſtors, which formerly frequented the borders of lake Brientz, and along the Arr towards Thun. I am certain M. Bourrit is miſtaken when he ſays that he ſaw one traverſe the chain of mountains that ſeparates Lauterbrunn and Grindelvald, ſince none of the hunters of that country ever fell in with any during their excurſions. It is aſſerted that wild goats are ſtill found, but rarely, in the wildeſt mountains of the country of the Griſons: for my part, I never ſaw any in my travels, and none of the hunters were able to give me any certain information reſpecting them."—*Letter of M. Wittenbach to M. Van Berchem jun.* Berne, May 27, 1785.

addreſs, a motive, indeed, of little importance to a being who thinks himſelf the ſovereign of animals, but who is only their tyrant, and knows no other laws than thoſe of the ſtrongeſt, the hunters ought to conſider, before they leave their wives and families, that they not only expoſe themſelves to be killed, or maimed during the remainder of their lives *, but neglect their property and affairs, and, if they periſh, their families are reduced to beggary. If theſe conſiderations are not ſufficiently ſtrong to deter them from this kind of hunting, let them reflect, that they can entertain no hope of making their fortunes; ſincé no one acquires riches by this occupation, but, on the contrary, they are often lamed, and drag out their exiſtence in miſery. But ſuch is the force of habit and the power of illuſion, that the accidents which happen to the hunters do not deter them; for they are ſcarcely cured when they return to the ſame occupation †.

The wild goat of Siberia, as may be ſeen by the deſcription of Pallas ‡, ſeems to differ from that of the Alps only by a few

* The elegant tranſlator of Coxe expreſſes himſelf in the following manner in regard to hunting the chamois:—"A great number of men loſe their lives by this kind of hunting: the thick fogs of the Alps, which ſuddenly envelop the country with a dark veil, make the hunters wander into the glaciers, where they periſh with cold and hunger. Sudden ſtorms moiſten the rocks, and render them ſo ſlippery, that the hunter, however furniſhed with cramps his ſhoes may be, cannot attach himſelf to them; and ſometimes the heat has ſo dried their ſcorching faces, and covered them with duſt of ſo moveable a nature, that the wretched hunter who clambers up them is obliged to moiſten them with his blood, by making large wounds in the ſoles of his feet and his legs." *Coxe's Letters on Swiſſerland*, p. 273. M. Ramond, the tranſlator, adds, that the abbé d'Engelberg congratulated himſelf that he had loſt only five men in one year by hunting the chamois; and his diſtrict is, at moſt, but a hundredth part of the Swiſs Alps.

† One of my friends traverſing the mountains one day with a guide, the latter ſhowed him a ſpot where the year before he had remained more than ſix hours with his thigh broken, and without any hopes of relief. A young ſhepherd, by the greateſt accident, heard the reports of his fuſee, which he fired as a ſignal of diſtreſs, and at laſt went to call aſſiſtance. This accident happened to him while hunting the chamois, and yet it did not diſguſt him with the occupation.

‡ The wild goat of Siberia is no longer found in the Uralian Alps and the civilized countries of Siberia: it inhabits the ſteepeſt mountains of that chain, which unites with the Taurus between Eaſtern Tartary and Siberia. It is found alſo in Eaſtern Siberia beyond the Lena, and in the countries of Kamſchatka. It is believed likewiſe that it frequents the deſert foreſts bordering on the middle country of the Tonguſians, to the eaſt of the Jeniſſee and beyond the Lena; but theſe animals are every where few in number, and frequent the moſt inacceſſible places. *Spicil. Zool.* faſc. xi. p. 32. 52. The old goats in that country are called *tæghé* or *teke*; the females *himæ*, and the kids *bitſchinja*. But M. Pallas thinks that theſe

a few ſlight characters, which, however, are not ſufficient to make them two diſtinct ſpecies. The former has always a very large beard; while that of the latter is very ſhort, and ſometimes it has none. The hair of the Siberian wild goat ſeems, in general, to be longer and thicker than that of the Alpine wild goat; but the moſt ſtriking difference, though not very ſpecific, is that found in the horns. The anterior face of the horns of the Siberian wild goat is not ſo flat, it is more convex; and is not comprehended between two longitudinal ridges, as in the wild goat in queſtion: the tranſverſal knotes are not terminated by a ſalient tubercle on the internal border of the face. In other reſpects, theſe horns have a perfect reſemblance.

The Cretan wild goat, according to Belon, is certainly of the ſame ſpecies as the Alpine wild goat: but it appears that there are two kinds of it, one of them perhaps the *ægagrus* *.

The chamois is much more common than the wild goat, though it is hunted ſtill more; but it is more widely diſperſed, as it inhabits the leſs elevated mountains; beſides, being more reſtleſs and ſhier than the wild goat, it is more difficult to catch it. It is found in all the high mountains of Swiſſerland, the Valais, and Savoy.

M. Perroud has given a very good account of the manners of this animal, which has been inſerted in Buffon's Natural Hiſtory †: we ſhall add to it a few obſervations. We are aſſured that in the mountains there are two ſpecies of the chamois: one, which is much ſmaller than the other, keeps in the higher diſtricts, and aſcends to the ſame elevations as the wild goat. The large ſpecies, on the other hand, frequent the leſs elevated regions, and live chiefly in the woods. Theſe two ſpecies, however, are only two varieties, or rather two conſtant races, which can mix and produce. The large chamois have two holes behind their horns, which are not found in the ſmaller kind. Theſe holes penetrate to the organs of the head. The males of this large ſpecies weigh from 80 to 100 pounds, and the females from 60 to 70; whereas the males of the leſſer ſpecies weigh only 60 or 70 pounds,

theſe names are corrupted from thoſe which the Moguls and the Calmoucs give equally to the wild goat and the domeſtic goat. The latter call the male *takja*, the female *jama*, and the kid *eſſiga* or *idſhija*. The Chineſe alſo call the male wild goats *takja*; and they employ their horns, as the Turks do, for their war-bows. The ſhavings of theſe horns are uſed in medicine. *Id.* p. 50.

* We found two different kinds, as we made appear by the diverſity of their horns, brought from Cyprus and Crete, which we preſented to M. John Choul, bailiff of the mountains of Lyons, &c.—*Belon*, fol. 14.

† Vol. xii. p. 160.

and the females no more than 40 or 50. The large kind are fond of being alone, and in ſmall flocks. Their limbs and body are ſhorter, and conſequently thicker in proportion than the ſmall kind, which are longer and ſlenderer.

The males can produce at the age of two years. The rutting time is in November and December, and they bring forth their young in June. The large chamois bring forth ſometimes in May. They generally produce one at a time, and rarely two. Theſe animals are hunted in the ſame manner as the wild goats, except that dogs are ſometimes employed. They are always in fear; have their eyes and ears on the watch. They never feed in tranquillity. They crop a little graſs, then raiſe their heads; and while they eat, look on all ſides around them: if one of them perceives any thing, the whole flock take to flight; whereas a flock of wild goats all whiſtle before they ſet off. In regard to the ſentinel which, it is ſaid, they poſt to give them notice of danger, it appears to be a mere fable, founded on the timidity of theſe animals. Both races of the chamois exiſt in the mountains of the country of Haſli, of Grindewald and Lauterbrun, as well as in thoſe of Savoy and the Valais. It might here be proper to examine, with Meſſrs. Buffon and Pallas, to what wild animal the goats are indebted for their origin; whether to the wild goat of the Alps, as Buffon thinks, or to the *capra ægagrus*, according to Pallas; and whether the chamois and Alpine wild goat are the ſame, or two diſtinct ſpecies. Theſe intereſting queſtions would require to be examined at full length *, but in the mean time I ſhall mention my own opinion. I have endeavoured to examine thoſe of Buffon and Pallas with that impartiality which every one who ſincerely ſearches for truth ought to employ, and eſpecially when he diſcuſſes the opinions of men ſo juſtly celebrated. I agree therefore with Buffon, that the Alpine wild goat ought to be conſidered as the origin of all the goats. The conformity of their manners and figure, and the circumſtance of theſe breeds intermixing and producing, are ſufficient proofs of it. In my opinion, the *capra ægagrus* and the capricorn are varieties in the ſpecies of the wild goat, and have concurred with it to form the different races of our goats. But, I think, with M. Pallas, that the chamois cannot be of the ſame ſpecies as the wild goat, ſince they do not mix in a ſtate of nature, and I conſider it as an intermediate ſpecies between the goats and the antelopes.

* I diſcuſſed this queſtion in the natural hiſtory of the jackall, which appeared in the *Journal de Phyſique* for November 1786.

XLII. *On*

XLII. *On the Spiral Vibration of a Stick or Rod.* By G. F. F. Chladni *.

THE vibrations hitherto known of a ſtick or rod, that is to ſay, of an elaſtic body extended in a certain direction, are either tranſverſal vibrations, where the rod or the parts into which it divides itſelf vibrate ſidewards, in ſuch a manner as to form a great number of curved lines; or longitudinal vibrations, where the rod or each of its parts contracts and extends itſelf according to the direction of its length, ſo that it ſometimes reſts againſt one and ſometimes againſt the other of the vibration nodes. The former kind of vibrations were firſt determined by Daniel Bernoulli, and afterwards, in a more complete manner, by Euler, in the Tranſactions of the Imperial Academy of Sciences for 1769: but the latter were firſt made known by myſelf in a paper on the longitudinal vibrations of rods and ſtrings, publiſhed at Erfurt in 1796, and which may be found alſo in the Tranſactions of the Electoral Academy of Sciences of that city. For ſome time I have obſerved, that, beſides theſe two directions of vibrating motion, there is a third, where the rod, or each part into which it divides itſelf, turns alternately to the right and left in a ſpiral form, while the vibration nodes or the boundaries of the vibrating parts remain motionleſs, as in the other kinds of vibration. Such ſpiral vibrations may be produced with moſt eaſe on a ſufficiently long cylindric rod, the ſurface of which has been made as ſmooth as poſſible, if the rod be held gently between two fingers at the place where there is a vibration node, and if it be rubbed in a ſpiral direction in a vibrating part with a piece of woollen rag held between the fingers of the other hand. When the rod is of wood or metal, the rag muſt be previouſly ſtrewed over with reſin; but when a glaſs rod, or, what is the ſame thing, a thermometer or barometer tube is employed, it muſt be moiſtened with water, or be ſtrewed over with very fine ſand. The ways in which a rod, when it is either entirely free, or faſtened at one end and free at the other, or faſtened at both ends, can divide itſelf into vibrating parts, and the poſition of the vibration nodes, are entirely the ſame as in the caſe of longitudinal vibrations. The ſharpneſs and flatneſs of the tone depend alſo on the ſame laws; and therefore, to avoid all unneceſſary prolixity, I ſhall refer to the above-mentioned eſſay

* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. ii. 1799.

on longitudinal vibrations. There is this difference, however, that, as far as I have obſerved, the tone, when the vibrations take place in a ſpiral direction, is a fifth lower than when the rod vibrates in a longitudinal direction under the ſame circumſtances.

By the ſpiral vibration one phænomenon, which I have mentioned in my diſcoveries reſpecting the theory of ſound, but reſpecting which I formed an erroneous opinion, can be explained. On a priſmatic rod, one end of which was faſtened in a vice, when I rubbed one of its edges, in a diagonal direction, with a violin bow, and ſtrewed ſand over one of its horizontal ſides, there appeared on this ſide a line proceeding along its length, where the ſand, which was thrown from the other parts by the vibration, remained at reſt; and this appearance took place on each ſide of the rod when held horizontally. The reaſon of this is, becauſe at the edges which are further diſtant from the axis the range during the vibrations is greater than in the middle of each ſide; and on this account the ſand which is thrown from the nearer places to thoſe on the edges muſt accumulate themſelves longitudinally in the middle of each ſide neareſt the axis where the vibrations are weakeſt.

XLIII. *Obſervations on Maddering; together with a ſimple and certain Proceſs for obtaining, with great Beauty and Fixity, that Colour known under the Name of the Turkey or Adrianople Red.* *By* J. M. Haussmann.

[Concluded from p. 175.]

The Proceſs.

After making a cauſtic ley of one part of good common potaſh diſſolved in four parts of boiling water, and half a part of quicklime, which I afterwards ſlaked in it, I diſſolved one part of powdered alum in two parts of boiling water; and while this ſolution of ſulphate of alumine was ſtill warm, to avoid re-cryſtallization, I ſpeedily poured into it ſucceſſively, always ſtirring it without interruption, the above-mentioned cauſtic ley, till the alumine it had at firſt precipitated after ſaturation to exceſs with ſulphuric acid had been rediſſolved. I left at reſt this ſolution of alumine, which exhaled ammonia, and which, on cooling, formed a precipitate of ſulphate of potaſh in very ſmall cryſtals. I then mixed a thirty-third part of linſeed-

linſeed-oil, with which the alkaline ſolution of alumine formed a kind of milky liquid *. As the oil gradually ſeparates itſelf from this mixture under the appearance of cream, it muſt not be employed till it is again ſhaken. The ſkains of cotton or linen ought to be ſucceſſively immerſed in it, and equally preſſed, that they may be then expoſed to dry on a pole in the order in which they have been taken from the mixture. They muſt be dried under ſhelter from rain in ſummer, and in a warm place in winter, and be left in that ſtate for twenty-four hours: they muſt then be waſhed in very pure running water, and be again dried; after which they are to be immerſed in an alkaline ley, preſſed and dried a ſecond time in the ſame manner as the firſt, taking care, however, to recommence the immerſion in the ley with thoſe ſkains which have been laſt in the oily mixture, becauſe the firſt never fail to carry away a larger portion than the laſt: it will be proper alſo to conſume the mixture each time, that it may not have leiſure to attract the carbonic acid with which the lower region of the atmoſphere is always charged, eſpecially in manufactories; for the alkali, by paſſing to the ſtate of carbonate, ſuffers the alumine to be precipitated, and loſes the property of mixing with the oil.

Two immerſions in the alkaline ſolution of alumine mixed with linſeed-oil will be ſufficient to obtain a beautiful red; but, by continuing to impregnate the ſkains a third and even a fourth time with the ſame circumſtances as the firſt, colours exceedingly brilliant will be produced.

The intenſity of the red propoſed to be obtained will be in proportion to the quantity of the madder employed. By taking a quantity of madder equal in weight to that of the ſkains the reſult will be a red, which, by clearing, will be changed to a roſy ſhade: on the other hand, ſhades of crimſon, more or leſs bright, will be obtained by employing two, three, and even four times the weight of madder, without ever forgetting the addition of chalk, if the water employed does not contain ſome of it. Four parts of this colouring ſubſtance will produce a red too intenſe and beautiful to be employed in commerce, as it would be too dear to find purchaſers.

By making an oily alkaline ſolution of alumine with two or three parts of water, and impregnating the ſkains twice, and even four times, in the manner above mentioned, bright ſhades will be produced without the uſe of much madder; but they will not have the ſame intenſity as thoſe procured

* In fact, a ſaponaceous liquor is formed containing alumine.—Edit.

with even as little madder by means of the ſame ſolution concentrated.

The beſt method of obtaining ſhades lively as well as bright, is to expoſe the dark reds for a conſiderable time, when they have been cleared, to the action of a ley of oxygenated muriate of potaſh, or of ſoda with exceſs of alkaline carbonate, in order to have ſuch a degree of ſhade as may be required: but it may readily be conceived that this method would be expenſive.

To have the oily alkaline ſolution of alum nearly in the ſame ſtate of concentration, it will be neceſſary to employ an areometer to determine the degree of ſtrength of the cauſtic ley before it is employed for the ſolution of the alumine. This cauſtic ley muſt be made with the beſt common potaſh that can be procured, and the degree it gives by the areometer muſt be noted, in order that, if potaſh of an inferior quality be afterwards employed, the ley obtained may be carried to the fixed degree by evaporation.

Cauſtic ley made with four parts of good common potaſh* cannot contain a large quantity of foreign ſalts. By making it on a large ſcale, when the limpid part has been decanted, it will be neceſſary to ſhake the depoſit, for ſome time, twice every day, that the reſt of the alkaline liquor may be decanted; and that none of what ſtill remains in the depoſit may be loſt, it ought to be diluted with more water, which may be afterwards employed to lixiviate the cotton, which muſt be well purified and cleaned before it is dyed; which may be done by lixiviating and ſoaping, or merely boiling it in water and then rinſing and drying it. As wringing with the hands may derange the filaments of the ſkains of cotton and linen, and conſequently weaken the thread, it will be proper, in operating on a large ſcale, to ſqueeze them by means of a preſs.

In regard to thread or linen to be dyed of a beautiful dark and fixed red, it muſt be well bleached, and impregnated at leaſt four times ſucceſſively with the oily alkaline ſolution; becauſe, not only alumine and metallic oxides adhere with more difficulty to linen than to cotton, but becauſe theſe mineral ſubſtances, when coloured, abandon linen much eaſier than cotton when clearing. It ſtill remains to examine whether, between each impregnation with the oily alkaline ſolution of alumine, cotton or linen thread requires to be left at reſt for a greater or ſhorter time before it is rinſed and dried.

* I have no doubt that, where potaſh cannot be procured, ſoda might be employed.—THE AUTHOR.

All

All fat oils may be employed in the mixture with proper precautions; but linſeed-oil mixes better, and remains longer ſuſpended in the alkaline ſolution of alumine: I never tried fiſh-oil, which, perhaps, would be preferable. It is probable alſo, that in operating on a large ſcale, it would be beſt to diminiſh the quantity of linſeed-oil in the mixtures with the alkaline ſolution of alumine; for I have had reaſon often to obſerve that too much oil hurts the attraction of the colouring parts of the madder: a thirty-third part of linſeed-oil always produced the beſt effect in my trials on a ſmall ſcale.

In regard to the proceſs of dyeing cotton and linen thread, ſufficiently charged with alumine, by the oily alkaline ſolution of that earth, the ſkains muſt firſt be diſengaged from every ſaline ſubſtance, as well as from the ſuperfluous oil, by rinſing them a long time in very pure running water; after which they muſt be arranged, without drying them, on an apparatus, which the operator may conſtruct according to the form of the boiler,—in which it is to be placed in ſuch a manner, that during the proceſs of dyeing the ſkains may be continually ſhaken and turned, in order to catch every where, and in an uniform manner, the colouring particles. The bath muſt be compoſed of madder, mixed with a ſixth of pounded chalk, and diluted with about 30 or 40 parts of water. The heat muſt be carried only to ſuch a degree that the hand can be held in the bath for an hour without being ſcalded; and it is to be maintained at this degree for two hours, either by diminiſhing or increaſing the fuel. Three hours dyeing will be ſufficient to exhauſt the madder: the ſkains when taken from the bath muſt be waſhed in a large quantity of water to cleanſe them; they are then to be cleared by boiling them a pretty long time in water containing bran incloſed in a bag, adding ſoap and alkaline carbonate to give the red a roſy or carmine ſhade.

As I never had occaſion to dye cotton or linen thread on a large ſcale, I employed a ſmall boiler, which ſerved me at the ſame time for the proceſs of clearing: in the latter operation I confined myſelf to boiling the ſkains, properly arranged, in water containing a bag filled with bran, for eight hours ſucceſſively; and, that I might not interrupt the ebullition, I replaced the evaporated part by the addition of more boiling water. In this clearing I employed neither ſoap nor alkali; yet I obtained a red ſuperior in beauty and fixity to that of the Levant, and which, in every reſpect, will bear a compariſon with the beſt colours dyed in France.

For dyeing my red, I employed three parts of the beſt madder for one part in weight of dry cotton thread.

With the precaution I took to obtain an uniform ſhade I could have dyed at one time, but I ſhould always recommend performing this operation at two different times, taking each time half a portion of madder and of chalk, if the ſkains cannot be continually turned in the boiler: it may ſerve alſo for clearing, by adapting to it a cover ſo as to ſuffer very little of the vapours to eſcape, becauſe it would be too expenſive to replace the part evaporated by more boiling water. By operating on a large ſcale, and concentrating the heat in the boilers, keeping them almoſt cloſe, there, perhaps, would be no need of employing eight hours ebullition to clear and fix the colours. I have every reaſon to believe that this clearing of the Turkey red gave riſe to the idea of bleaching with ſteam: it muſt have been ſeen that colours by being cleared loſe conſiderably in regard to their intenſity; and perhaps it has been obſerved at the ſame time that the packthreads employed for arranging the ſkains were bleached during the clearing, eſpecially when alkalies were added.

A great variety of colours and of different ſhades may be obtained by following the proceſs here deſcribed for obtaining beautiful and durable reds. In this caſe, the oily alkaline ſolution of alumine muſt not be employed till the required ſhade of oxide of iron or indigo blue has been given; but whatever may be the colour or ſhade which you wiſh to give, before you fix the alumine on the ſkains of cotton or linen, theſe ſkains muſt always be firſt well boiled, by which means the adheſion of the indigo fecula as well as that of the oxide of iron will be increaſed in the ſame manner as that of alumine coloured by the colouring parts of madder when ſubjected to the action of the heat of boiling water before they are impregnated with the oily alkaline ſolution of alumine. As the method of dyeing indigo blue in all its ſhades is well known, it is needleſs to detail it; and as to giving a ruſty yellow colour, which may be done at little expenſe, nothing is neceſſary but to moiſten the ſkains well with a ſolution of the ſulphate of iron, to preſs them equally, and then to immerſe them in a cauſtic ley of potaſh, which will precipitate and fix the oxide of iron of a diſagreeable colour, but which will not fail to aſſume a ruſty yellow ſhade by attracting and becoming ſaturated with the oxygen of the atmoſphere: thus yellow will be more or leſs dark according to the quantity of the ſulphate of iron in ſolution. More intenſity and even more equality may be given to the ruſty yellow by moiſtening the ſkains a

ſecond

ſecond time in the ferruginous ſolution, and immerſing them in the cauſtic ley. Care, however, muſt be taken not to uſe ſoda for this operation, becauſe it generally contains ſulphur, which blackens oxide of iron by mineralizing it.

The ſkains coloured blue and ruſty yellow, treated with oily alkaline ſolution of alumine, will produce, by maddering, dark purple and chamois colours, violet, lilac, puce, mordoré, &c. It may be eaſily conceived, that if, inſtead of maddering, the ſame ſkains prepared for maddering be dyed with kermes, cochineal, and Brazil wood, logwood, wood of St. Martha, woad, yellow wood, quercitron, yellow berries, &c. a great variety of colours will be obtained: the ſhades may even be varied *ad infinitum* by mixing the colouring ingredients with each other in different proportions. The affinity of adheſion of the colouring parts of all theſe ingredients varies alſo to ſuch a degree, that the ſhades ariſing from a yellow or olive-green will be changed or totally metamorphoſed by a ſecond dyeing with madder, kermes, cochineal, or Brazil wood; and will furniſh orange ſhades, capucine, carmelite, burnt bread, bronze, &c. As the preliminary preparation of the ſkains by the oily alkaline ſolution of alumine might be too expenſive for ſome of theſe colours, the proceſs I deſcribed in the *Annales de Chimie* for the year 1792, p. 250, may be ſubſtituted in its ſtead. This proceſs conſiſts in treating the ſkains alternately with ſoap and ſulphate of alumine, the exceſs of the acid of which has been ſaturated with one of the alkaline carbonates or with lime: this method is very expeditious. In the courſe of a day, eſpecially in ſummer, the ſkains may be prepared and dyed red as well as other colours; which, for the moſt part, may be ſubjected to ebullition, and will bear clearing with bran for a quarter or half hour, and even ſome of them for a whole hour. It is alſo to be obſerved that there are none but madder colours, the alumine and oxide of iron baſes of which have been fixed on the ſtuffs by means of the oily alkaline ſolution, that can acquire perfect fixity by the action of heat of boiling water; and that the fixity is very inferior in all madder colours the earthy and ferruginous baſes of which have been applied to ſtuffs by means of acid ſolvents.

Alumine, fixed in abundance on cotton or linen ſtuff by means of a highly concentrated alkaline ſolution, attracts very eaſily the colouring parts in the proceſs of maddering. The caſe is not the ſame when the ſame earth is applied by the moſt highly concentrated acetic ſolution of alumine; and it is abſolutely impoſſible to finiſh maddering at one time, even when a profuſion of madder is employed, and the operation

tion is repeated three and even four times *. This circumstance will give rise to new and interesting experiments; but my observations prove in the mean time that maddering, in general, requires to be managed with the nicest attention.

XLIV. *A new Method of separating Silver alloyed with Copper in Bullion.* By C. Napione †.

METALLURGISTS well know that argentiferous copper, which does not contain half its weight in silver at least, cannot be subjected to cupellation. In that case, recourse is had to an operation, called *liquation*, with lead; but, unhappily, it is attended with inconveniences which in many cases render it impracticable.

To extract silver from copper completely, in one liquation, practice has shown in the founderies where this operation is performed on a large scale on black copper, that for a quintal of the latter there must be at least 70 or 80 pounds of copper, and more than 8 or 9 ounces of silver; and in this case the proportion of lead to the copper ought to be 11 parts of the former and 3 of the latter.

But as in *billon* (base) money the silver generally forms from 1-12th to 1-4th of alloy, and sometimes more, it is evident that several liquations must be repeated to impoverish the copper; for though in the first the lead carries with it a great deal of silver, it is not always in the same proportion, and the latter portions are more difficult to be extracted.

If we suppose that four successive liquations are sufficient to extract the silver of our *billon* money, which is of the quality of 3 deniers 10 grains, estimating it at the lowest, we should have 55 livres for the whole expense of the separation for each quintal of that money.

If the process of the celebrated Cramer, of which M. Jars has given a short account in the third volume of his *Voyages Metallurgiques*, can be attended with any advantage, it would, however, be of very little use to us.

C. Napioné, knowing that sulphur has a greater affinity for copper than for silver, conceived the idea of reducing into scoriæ a large quantity of rich copper by combining it directly with sulphur, in the hope of obtaining, by this kind of dry *depart*, the silver concentrated in a portion of the copper

* Concentrated acetic solution of oxide of iron is attended with nearly the same difficulties.

† From the *Annales de Chimie*, No. 122.

so

ſo as to be able to refine it immediately by cupellation, and to obtain at the ſame time ſcoriæ, leſs rich, for amalgamation.

The experiments which he relates prove that he was not deceived in his expectation. He took a pound and ſix ounces of argentiferous copper of the quality of $3\frac{1}{2}$ deniers the marc, and, having fuſed it in a crucible, mixed with it two ounces of ſulphur. Having ſeparated the *matte* or ſcoriæ, he repeated the ſame operation twice on the *cupreous regulus*; and after the three operations he obtained 1 pound 5 ounces and 22 deniers of *matte*, and 4 ounces 16 deniers and 12 grains of metallic regulus, which, though pretty ductile, ſplit at the edges when flattened with the hammer.

The trial made on *mattes* or ſcoriæ of three meltings united, ſhowed that they contained only 11 pounds 3 ounces of ſilver in the docimaſtic quintal, and the quality of the regulus was 7 deniers 16 grains per marc; a quality at which it may be ſubjected to cupellation without any other proceſs.

After having well pulverized the *mattes*, and mixed with them muriate of ſoda in the proportion of 12 to 100, and the ſame quantity of quicklime, and after calcining it for four hours under a muffle, he amalgamated the mixture. The reſult of this amalgamation and of a ſubſequent one procured him only a very ſmall quantity of ſilver. He again calcined it, for three hours and a half, with 1-10th muriate of ſoda; and after three calcinations of the ſame kind under the ſame circumſtances, and three alternate amalgamations, his *mattes* contained no more than 1 ounce and 18 deniers of ſilver per quintal.

C. Napioné obſerves, that if his firſt calcination had been carried further, he could have extracted the whole of the ſilver in three amalgamations. Having repeated, indeed, the ſame operation at the mint, employing the ſame proportion of ſulphur, he was able, after three ſucceſſive calcinations and three amalgamations, to obtain a reſiduum, which contained only an ounce and a half of ſilver per quintal. The ſilver extracted on this occaſion was of the quality of 11 deniers and 22 grains. It was of importance, after this laſt trial, to undertake the experiments on a great ſcale.

On a hearth prepared with light *braſque**, as for the refining of common copper, and which C. Napioné had placed in a ſituation where there was a current of air, a baſon was dug capable of containing about a quintal and a half of melted matter: the chimney was diſpoſed in ſuch a manner,

* A lining or coating of a mixture of clay and pounded charcoal.—EDIT.

that

that a proper degree of heat could be given to the baſon and the fuſed metal. The *braſque* being very dry, a quintal of our *billon* money was fuſed in the baſon through charcoal; and the charcoal being then removed from the top, a workman, furniſhed with a long iron ſpoon, threw ſulphur on the melted maſs, while another workman ſtirred the matter with a clay rod. In proportion as ſulphuret of copper was formed, the ſurface of the maſs was ſprinkled with a bruſh dipped in water, and another workman, with an iron fork, removed in plates the *mattes* as they became fixed. There was obtained in this manner, after the operation, a cupreous button, and *mattes* nearly in the ſame proportion, and containing the ſame quantity of ſilver, as in the trials on a ſmall ſcale. A quintal and a half of matter was thus fuſed at once, and it is probable that a greater quantity might be fuſed in the ſame manner.

The *mattes*, well pulverized, were ſubjected to calcination with muriate of ſoda and lime in a cupelling furnace arranged for that purpoſe.

After a calcination of ten hours, the matter was amalgamated in one of thoſe mills commonly uſed in the mint for extracting ſilver and gold from the refuſe of different operations. After the firſt amalgamation, which laſted only 14 hours, the third part of the ſilver contained in the oxide was extracted; and, as it is in the ſecond amalgamation that the greater part of the ſilver is attacked by the mercury, we may conclude, without any danger of being deceived, that three amalgamations will be more than ſufficient to extract, on a large ſcale, the whole ſilver of the *mattes*.

Theſe mills, which contain from 26 to 30 pounds of matter, may be put in action, to the number of forty, by a ſingle water-wheel: beſides this advantage, their capacity may be augmented.

By the proceſs of C. Napioné, not only ſilver but the gold united with it may be extracted from argentiferous copper; and this may be done by one liquation.

As the water of the waſhing of the firſt amalgamation contains a great deal of ſulphate of copper, the metal may be extracted from it by cementation with iron, or by precipitating it by carbonate of potaſh, which will produce cupreous carbonate of a bright green colour. The ſame water afterwards evaporated gives ſulphate of ſoda in abundance.

The water of the ſecond waſhing and the third amalgamation is colourleſs; and there may be extracted from it, with advantage, by evaporation, muriate and carbonate of ſoda.

The oxide of copper freed from the ſilver may be ſent to the

the ſmelting-houſes at the copper-mines to be reduced along with the ores, and refined by the common method.

C. Napioné, having calculated the expenſe neceſſary for his proceſs when executed on a large ſcale, concludes that, in the preſent circumſtances, it will never amount to a fifth part of that which refining by common liquation requires.

XLV. *Deſcription of the Duke of Bridgewater's Drain Plough* *.

SIR,

I AM favoured with yours of the 15th inſtant. The model of the guttering-plough ſent you, was a preſent to the Society from his Grace the Duke of Bridgewater, made by Robert Tomlinſon, one of his Grace's conſtant workmen.

I procured the following account this morning from the duke's farmer:—In clay or ſtiff land that lies flat, the plough cannot go too deep; but if it lies on a declivity, about five inches deep is ſufficient. In ſoft light ſoil, the plough ſhould go as deep as it can in all ſituations, becauſe the ſides moulder into the gutters. The beſt time of draining is about Michaelmas, or as ſoon as the graſs is eaten; and the whole ſhould be accompliſhed betwixt that time and Chriſtmas.

In clay ground that has never been drained, ſix good horſes will be requiſite to draw the plough. In every following year the plough ſhould be run through the ſame gutters, and four horſes will then be ſufficient. I am, Sir,

Your moſt obedient ſervant,

THOMAS BURY.

P. S. The ſhare of the plough muſt be well ſteeled, and ſhould be ground ſharp.

Worſley,
September 18, 1800.

Mr. Charles Taylor.

*** At Broughton, in the neighbourhood of Mancheſter, conſiderable quantities of ſtiff clay, paſture, and meadow land, have been much improved, under the inſpection of the Secretary of the Society, by the uſe of this plough.

After the cattle were houſed for the winter, three horſes

* From the *Tranſactions of the Society for the Encouragement of Arts, &c.* 1801.—The thanks of the Society were this ſeſſion voted to his Grace the Duke of Bridgewater for a model of this drain plough, preſented by him to them, and ſent with the above communication from his Grace's agent, Mr. Thomas Bury, of Worſley Mills, near Mancheſter. The model, which is made upon a ſcale of one inch to a foot, is placed in the Society's repoſitory for public inſpection.

were

were employed to form drains with the plough at proper intervals: the ſmall drains were made at the diſtance of about nine yards from each other, in old furrows of the ground, and about five inches deep: the ſod, when cut out by the plough, was of a wedge-like form, and turned out by it upon the ridges of the land, entirely ſeparated from the drain or gutter. Theſe ſods were afterwards divided acroſs, by a ſpade, into lengths of about two feet each, then toſſed by a pitchfork into a cart, and placed in a heap in the field, along with ſtrata of quicklime in a powdery ſtate: the whole maſs was reduced to a compoſt by the froſt during the winter, and in the following ſpring was laid upon the ſurface of the land, and formed an excellent top-dreſſing.

The water from the ſmall drains was directed into larger drains, made by lowering the ſhare of the plough to the depth of nine or more inches. Little or no loſs of land aroſe from the ſmall drains, as natural graſſes were produced therein early in the ſpring. It will be highly advantageous to repeat the operation every winter: it is eaſily and expeditiouſly performed; and no perſon, without an actual experiment of the fact, can form a ſufficient judgment of the great benefit ariſing to vegetation by the removal of cold ſtagnant water, during the winter, from land of every deſcription.

Deſcription of the Duke of Bridgewater's Drain Plough, (Plate VII. fig. 1.)

A B, the beam of the plough.

C D, the handles,

E, the ſhare or ſock.

F, the coulter, or firſt cutter of the ſod; which coulter is fixed to the ſhare.

G, the other coulter, or ſecond cutter, which ſeparates the ſod from the land, and directs it through the open ſpace betwixt F and G. This coulter is connected with the ſhare and the beam.

H I, the ſheath of the plough.

K, the bridle or muzzle, to which the ſwingle-tree is to be fixed.

L M, two wheels of caſt iron, which may be raiſed or lowered by ſcrews at N preſſing upon the flat irons O O, to which the axis of each wheel is fixed. Theſe wheels regulate the depth which the ſhare is to penetrate into the earth.

P, a chain with an iron pin, to move the ſcrews at O.

XLVI. *De-*

XLVI. *Description of Mr.* KNIGHT'S *Drill Machine for sowing Turnip-seed*.*

SIR,

I HAVE sent you a small instrument for sowing turnips, which I have tried on several different soils, and think I can venture to assert, that it will sow the seed and cover it perfectly well in any soil that is nearly in a proper state to receive it. It is necessary either to harrow the ground *across*, or to roll it, previously to the instrument being used, that the labourer may see the rows he has made: but I have always found the crop to succeed better after the roller than after the harrow, though the ground has been very strong.

The instrument is so extremely simple in its construction, that it is almost unnecessary to give a description of its mode of action; but as parts of it may probably be broken in carriage, I have added the following sketch: See Plate VII. fig. 2. —A, the iron wheel, which, running on its edge, formed by two concave sides, makes the groove into which the seeds fall. I have sometimes used a wheel with straight sides; but I think that concave sides, when well executed, are to be preferred in strong soils, and indeed in any soil. B is a wheel moving on the same axis with A, and turning the wheel C (which gives out the seed) by means of a strap. I have several sizes of the wheel B, in order to increase or diminish the rapidity of C; and consequently to sow more or less seed. D, the tube through which the seed passes, and falls into the channel made by the iron wheel. E, the feet of the instrument. F, six lengths of jack-chain; which I find cover the seed remarkably well. The chain is perhaps preferable to any kind of harrow, because it can never become encumbered by loose straw, which is almost always found on or close to the surface, when the ground has been manured; and the iron cutting-wheel has a similar advantage over any kind of share. G I, the seed-box. H H, the handles of the machine.

The labour of using the instrument is very small. My workman usually accomplishes four statute acres, or something more, in a day; and last night, with the one I send, he sowed an acre and a half after six o'clock in the evening. There are two holes before the axis of the great wheel, to receive two pieces of cane, which point out the proper width

* From the *Transactions of the Society for the Encouragement of Arts, &c.* 1801.—The silver medal was this session voted to Thomas Andrew Knight, Esq. of Elton near Ludlow, for this invention. A complete machine was presented by him, and is placed in the Society's repository.

of the intervals between the rows. I uſually place my rows at eighteen or twenty inches' diſtance; and I wiſh my plants to ſtand not more than ſix inches apart in the row; for I find that three ſmall turnips weigh about as much as one large turnip, are more ſolid, and I think more nutritious, and certainly are much leſs apt to ſuffer by unfavourable weather. The ground between the rows is, of courſe, worked with the hoe. Should the Society, at their next meeting, approve of the inſtrument, and will afford it a place in their repoſitory, I will beg them to accept it; if not, I will requeſt you to return it at your leiſure. I am, Sir,

Your moſt obedient ſervant,

THOMAS ANDREW KNIGHT.

Elton, near Ludlow,
June 22, 1800.

Mr. Charles Taylor.

N. B. The angle which forms the edge of the wheel A, muſt be made more or leſs acute, and the inſtrument more or leſs heavy, proportional to the ſtrength of the ſoil. I have ſometimes added weights of lead over the axis of the wheel, but it will rarely be found neceſſary. I have tried the inſtrument on different ſoils, and I think it will anſwer on any. A great advantage may be derived by ſowing turnips with it, at a time when horſes, now commonly uſed for the ſame purpoſe, are engaged in other employments. A few days are frequently of importance in ſowing turnips, which by fortunate rains have got a wonderful ſtart of thoſe which have been ſown a day or two later.

Fig. 3. is a ſection, on a larger ſcale, of the ſeed-box G, in fig. 2. The wheel marked C, is alſo the ſame as in that figure: it is fixed upon the axis of the cylinder I, which is pierced upon the ſurface with holes at K, for the ſeed. This cylinder turns round within a groove at the bottom of the box, and is ſo well fitted therein, that no ſeed falls from the box but what is delivered by the holes K. A ſmall bruſh, marked L, rubs againſt the cylinder, to clear out any ſeeds which may remain in the holes.

The ſeeds fall into the tube underneath the cylinder, and from thence into the channel made by the indenting rim of the iron wheel.

The looſe chains which follow, cover the ſeeds with earth, as before mentioned.

Fig. 4. a front view of the wheel, exhibiting its edge.

XLVII. *Notices respecting New Books.*

Manchester Transactions.

THE Second Part of Vol. V. has just made its appearance. The following are the contents:

On Tragedy, and the Interest in Tragical Representations: an Essay. By the Rev. George Walker, F. R. S. and Professor of Theology in the New College, Manchester.—Experiments and Observations to determine whether the Quantity of Rain and Dew is equal to the Quantity of Water carried off by the Rivers and raised by Evaporation; with an Inquiry into the Origin of Springs. By Mr. John Dalton.—Experiments and Observations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Essay on the same Subject. By Mr. John Dalton.—Experiments on the Velocity of Air issuing out of a Vessel in different Circumstances; with the Description of an Instrument to measure the Force of the Blast in Bellows, &c. By Mr. Banks, Lecturer in Natural Philosophy.—Essay on the Beautiful in the Human Form; and Inquiry whether the Grecian Statues present the most perfect Beauty of Form that we at present have any Acquaintance with. Communicated to the Society from a Correspondent through the Rev. George Walker.—A Defence of Learning and the Arts, against some Charges of Rousseau. In two Essays. By the Rev. George Walker, F. R. S.—Observations on the Nervous Systems of different Animals; on Original Defects in the Nervous System of the Human Species, and their Influence on Sensation and Voluntary Motion. By John Hull, M. D.—Experiments and Observations on the Heat and Cold produced by the Mechanical Condensation and Rarefaction of Air. By Mr. John Dalton.—Account of some Antiques lately found in the River Ribble. By Mr. Thomas Barritt.—Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat. By Mr. John Dalton.——A Review of some Experiments which have been supposed to disprove the Materiality of Heat. By Mr. William Henry.—An Investigation of the Method whereby Men judge, by the Ear, of the Position of Sonorous Bodies relative to their own Persons. By Mr. John Gough.—On the Theory of Compound Sounds. By Mr. John Gough.—Meteorological Observations made at Manchester.

chester. By Mr. John Dalton.—Appendix. I. Explanation of a Roman Inscription found in Castle-field, Manchester. By Mr. Thomas Barritt: with a Note on the same Subject by Dr. Holme.—II. Note to Mr. W. Henry's Paper on Heat.

Ausführliche Geschichte der Theoretisch Praktischen Uhrmacherkunst, &c. A History of Clock- and Watch-making, both Theoretical and Practical, since the earliest Method of dividing the Day to the End of the 18th Century. By I. H. Moriz Popee, 1801. 8vo. 564. p. 8.

[Continued from p. 179]

V. *The invention of clocks moved by wheels and weights, and their progressive improvement till the middle of the seventeenth century.*—The invention of clocks moved by wheels and weights, as well as the period of the invention, is totally unknown. The author, however, has collected the researches of learned men on this subject, and refutes the opinion of those who make the invention to be not older than the 14th century. The invention, however, was not very great, as the more perfect water-clocks were before that period furnished with wheels, so that the only improvement was the substitution of a solid body to act as a moving weight instead of water. The principal point was to produce uniformity in the action of these weights; and in this respect the first clocks moved by weights were deficient till the time of Huyghens. It is not therefore to be wondered at, that the application of weights to clocks as a moving power should excite so little attention as to be scarcely thought worth notice by cotemporary writers: water and sand were indeed more convenient than a solid weight, which requires too much room for its motion. The author is of opinion that the origin of clocks with weights is as old as the 11th century, and adduces very probable grounds in support of it; but it is not properly ascertained, whether the inventor was an European or a Saracen. The oldest complete clock moved by weights, of which there is any certain testimony, is that sent by the sultan of Egypt in the year 1232 to the emperor Frederic II, the value of which was at that time estimated at 5000 ducats. In the 13th century many of the church-steeples in Italy were furnished with clocks moved by weights that struck the hours. About the same time the well-known clock-house at Westminster-hall was furnished with a clock that struck the hours; the expense of which was defrayed by a fine imposed on one of the judges. The clock moved by weights of the abbot Richard of Wallingford, which by many has been considered as the oldest, was constructed in the 14th century. In the

year 1394 Padua obtained the first clock, supposed to have been constructed by the celebrated philosopher, physician, astronomer and mechanist James Dondi. That such clocks might be introduced into England, Richard III. gave a patent to three Netherlanders, in the year 1368. Courtrai in France had a clock that struck the hours so early as the year 1332: it was carried away in that year by Philip the Bold of Burgundy, and conveyed to Dijon, where it is still to be seen. The first large clock at Paris was erected in 1364 by a German artist, Henry von Wick, who received daily six French sous, together with free lodging in the tower of the Palais, to which the clock was removed in 1370. Bologna obtained the first public clock in 1356; Pavia, in 1402; Breslaw, by Schwelbelin, in 1368; Strasburg, in 1370; Augsburg, in 1398; Nuremberg, in 1462; and Venice, in 1497. The first clocks were expensive, and many cities, desirous of having such machines, were not able to raise money to purchase them. In the year 1523, the fines levied from the students at Oxford were employed to defray the expense of the clock erected on the church of St. Mary. Private individuals, however, about this time began to obtain clocks. The first instance of a clock with weights being employed for astronomical purposes occurs in 1484, when Walther, as he says, with a well regulated clock, made an observation of Mercury. Tycho had three of these clocks, which showed minutes and seconds: he found, however, that they were exposed to variations from the influence of the atmosphere and wind; on which account he caused to be constructed a quicksilver clock, in which distilled quicksilver, instead of sand and water, showed minutes and seconds. To produce an uniform fall, so much quicksilver dropped from another vessel into the hour-vessel as was sufficient to keep it always at the same height. As the study of astronomy required more accurate measurers of time, we are indebted to that science for the improvement of these instruments, the construction of which does so much honour to the human genius.

VI. *Invention of watches and table-clocks.*—Hele of Nurnberg is generally considered as the inventor of watches or spring-clocks, as they ought properly to be called; the first of which he is said to have constructed in the year 1500. According to others, Halbrecht of Strasburg was the inventor: but his first clock was constructed in 1520, and consequently twenty years later than Hele; who, it is certain, made small spring-clocks or watches so early as 1500. Nurnberg and Augsburg were the first cities of Germany in which watches were made. Hele's watches not only indi-

cated the hours, but ſtruck them alſo. Andrew Heinlein, who trod in Hele's footſteps, made ſmall clocks in the ſmelling-balls which were uſed in his time. In the architectural office at Augſburg there is a watch, above 200 years old, which ſtrikes the hours: it is contained in a cryſtal caſe, and was made by Buſchman. There were ſtriking watches in France in the time of Louis XI. The oldeſt watch in England, that goes ſtill pretty well, is of the year 1540, and is preſerved in the palace of Hampton Court.

VII. *Invention of the fuſee, pendulum, and ſpiral ſpring.*—The fuſee was in all probability invented in England about the end of the 16th century, by whom is not known, and was thence introduced into Germany. It was examined geometrically by Varignon and De la Hire, in order to determine the moſt advantageous form; which, however, does not always avail, on account of the inequality of the ſpring, and therefore advantage is taken of other mechanical means: of this kind is the balance, by means of which the power of the ſpring can be adjuſted, and which undoubtedly was invented in Swiſſerland about the beginning of the 18th century. In the firſt watches with fuſees, the diameter was ſmall, and the box large and broad. The catgut, by which it was wound up, paſſed eight or nine times around the fuſee. This made the watch clumſy and ill-ſhaped. The catgut, however, was ſoon exchanged for a chain, which conſiſts of ſmall plates of ſteel united together with great labour. In the middle of the 17th century, Huyghens invented a better method of regulating the movement of clocks. In the year 1657 he applied the pendulum as a regulator to large clocks moved by weights; and, ſome years after, recommended the balance ſpring for watches. Before that period ſome had uſed, inſtead of the ſpoon-formed balance, one in the form of a ring, or employed a ſmall fly wheel; and Hautefeuille uſed at firſt a ſwine's briſtle, and then a weak, ſtraight, ſteel ſpring, for regulating the balance. In the year 1674 Huyghens cauſed a watch with a ſpiral balance ſpring to be conſtructed by Turet at Paris. Dr. Hook entered into a diſpute with Huyghens in regard to this invention, and proved that he had invented ſuch a watch for Charles II., which had the inſcription *Robert Hook invenit* 1658, *Tompion fecit* 1675; and that he had ſolicited a patent for ſuch watches in 1660, but did not obtain it till 1675.

[To be continued.]

XLVIII. *Pro-*

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

ON the 1ſt of April a ſhort notice was communicated to the Society of ſome obſervation, made by M. Bode of Berlin, relative to the Ceres Ferdinandea. He makes the oppoſition of that planet to have been on the 17th of March laſt at $4^h\ 36'$ mean time.

The reſt of the evening was taken up in reading the remainder of Count Bournon's paper on corundum, noticed in our laſt. The reading was continued on the 8th, but without getting through the paper, of which, from its nature, being chiefly a deſcription of the forms of cryſtals, we can give no account in a ſhort notice.

On the 15th and 22d there was no meeting, owing to their falling in Paſſion week and the Eaſter holidays.

FRENCH NATIONAL INSTITUTE.

Account of the labours of the Mathematical and Phyſical Sciences during the ſecond quarter of the year 10.

Mathematical Part, read by C. Delambre, ſecretary.

Aſtronomy—Piazzi's Planet—New Lunar Equations.

About the beginning of December Baron Von Zach obſerved Piazzi's ſtar in a group of others; but, having nothing to diſtinguiſh it from them, he was not able to find it again, and to aſſure himſelf that he had ſeen it, till towards the end of that month. Dr. Olbers had obſerved it alſo at Bremen about the ſame time. The difficulty which aſtronomers experienced in finding it, muſt be aſcribed to its ſmallneſs and the unfavourableneſs of the weather; for, according to the elements which M. Gauſs deduced from the obſervations of Piazzi, an inſtrument could be eaſily directed to the altitude of this ſtar, and in two minutes it would have been in the middle of the teleſcope. Before we received this intelligence from Germany, we endeavoured to take advantage of every moment when the ſky was favourable. C. Mechain, whoſe reſearches were leſs interrupted, and who had obſerved all the ſmall ſtars among which the planet could reappear, perceived it, for a moment, on the 23d of January; but day having ſurpriſed him too ſoon, he could only examine in haſte the configuration of the ſtars which occupied the field of the teleſcope. On the 24th, however, he examined it with more accuracy. On the 25th he communicated to us his obſervation. There remained then no more difficulty. In

the evening, when the planet was diſengaged from the vapours of the horizon, I obſerved it with a parallactic inſtrument, and followed it for ſix hours without interruption to aſſure myſelf of its motion. By a great deal of attention I was able to ſee it on the meridian, where I miſſed it the preceding evening in conſequence of foreign circumſtances. After that period we followed it cloſely, and always obſerved it when the weather would admit; that is to ſay, about five times in thirteen days; for from the 25th of January to the 31ſt of March, I could ſee it only twenty-ſix times on the meridian. It now paſſes a few minutes before the Georgium Sidus; and this proximity facilitates a compariſon of the two planets with which aſtronomy has been enriched in our time. That of Herſchel appears as a beautiful ſtar of the fifth or ſixth magnitude, and may be eaſily diſtinguiſhed by the naked eye. That of Piazzi eſcapes the beſt eye, even when it attempts to find it in the place indicated by the teleſcope, in which it has been obſerved. It is indeed only of the ſeventh magnitude: owing, however, to an union of circumſtances in this ſeaſon, it appears in all its ſplendour; for it is at its neareſt diſtance to the earth, and at a ſmall diſtance from its perihelion. It was much leſs brilliant when found: it can never now be loſt.

C. Flaguergues, aſſociate, has ſent us obſervations he made at Viviers. Among theſe is a new determination of the latitude of that place, which he has found to be 18″ further north than marked in the *Connoiſſance des Tems*; alſo, obſervations of ſeveral ſpots, and particularly two which have reappeared at the ſame place after one or two revolutions of the ſun around his axis.

C. Laplace has communicated to the claſs his labours for improving the theory of the moon. He has announced that, inſtead of one equation of a long period, of which there was ſome reaſon to ſuppoſe the exiſtence, he has diſcovered two. This complication rendered the labour more difficult. Beſides, theſe analytical calculations are ſo embarraſſing, and depend on ſubſtitutions ſo delicate, that it is proper to call in the aid of obſervation to fix the preciſe value. But as the period of theſe new equations is more than 180 years, we cannot flatter ourſelves with the hope of knowing both with complete accuracy until one or more periods have been obſerved.

Experiments which prove that all bodies, of whatever nature, are ſubject to the magnetic influence, by C. Coulomb. The new experiments undertaken by Coulomb, and which he repeated before the Inſtitute, induce us to believe that the action of magnetiſm extends throughout all nature, ſince of all the bodies hitherto tried none of them has eſcaped the influence

fluence of the artificial magnet. But this action, though real, has not the ſame force in all bodies; and in the greater part it is neceſſarily very ſmall, ſince it has hitherto eluded the notice of philoſophers.

C. Coulomb gave to each of the bodies which he tried the form of a cylinder or ſmall bar: in this ſtate he ſuſpended them by a thread of ſilk, ſuch as it is when taken from the cod, and placed them between the oppoſite poles of two bars of ſteel. The ſilk thread can ſuſtain no greater weight than eight or ten grammes without breaking; it was therefore neceſſary to reduce to very ſmall dimenſions the needles formed of the different bodies ſubjected to experiment. C. Coulomb made them of from ſeven to eight millimetres in thickneſs: thoſe of metal he made only one-third of that ſize.

For theſe experiments he placed the bars of ſteel in the ſame ſtraight line, their oppoſite poles being diſtant from each other five or ſix millimetres more than the length of the needle which was to oſcillate between them. The reſult of theſe experiments has ſhown, that of whatever matter the needles conſiſted, they always arranged themſelves exactly in the direction of the two bars; and that, if removed from this direction, they always returned to it by oſcillations, the number of which was often thirty in a minute. The weight and figure of the needles being given, it was therefore eaſy to determine the force which produced theſe oſcillations.

Theſe experiments were made in ſucceſſion with ſmall plates of gold, ſilver, copper, lead, and tin; ſmall cylinders of glaſs, a bit of chalk, a fragment of bone, and different kinds of wood.

Phyſical Part, read by Lacepede, ſecretary.

Chemiſtry and Mineralogy.—C. Monge has given an account of ſeveral important obſervations in regard to the theory of the earth, and which he made during his travels in Italy and Egypt.

C. Ramon has given obſervations on the ſtructure of the mean and inferior mountains of the valley of Adour, one of the moſt intereſting in the Pyrenees.

C. Lamarck has publiſhed an intereſting work entitled *Hydrogeology, or Reſearches on the Influence which the Waters have on the Surface of the Earth, on the Cauſes of the Exiſtence of the Baſon of the Seas, its Diſplacement, &c.*

C. Seguin, aſſociate, read two memoirs reſpecting cinnabar; in which he endeavours to prove that ethiops and cinnabar are only compounds of ſulphur and mercury, without oxygen and hydrogen: that theſe two ſubſtances differ from

each other only in the proportion of their principles, and particularly in the degree of the union of their moleculæ; that this proportion and this degree of union are invariable in the cinnabar, and, on the other hand, very variable in the ethiops: and, in the laſt place, that cinnabar is compoſed of thirteen parts and one-third of ſulphur, and eighty-ſix parts two-thirds of mercury.

C. Seguin has written a third memoir on colophonium. After having ſhown that good colophonium is nothing but reſin completely purified from eſſential oil, and from which a portion of its hydrogen has been taken; and after having proved that the colophonium uſed in commerce is more or leſs deficient, he indicates a new proceſs for obtaining it of a good quality.

Mineralogiſts had hitherto conſidered the *oiſanite* or *anataſe* of Haüy as a peculiar kind of ſtone. But Vauquelin, by ſubjecting this mineral to analyſis, has found that it is cryſtallized oxide of titanium. The *anataſe*, therefore, muſt hereafter be claſſed among metals, and in the *titanium* genus. However, as the form of this mineral is not the ſame as that of native oxide of titanium, Vauquelin thinks there is reaſon to preſume that the anataſe holds in combination ſome ſubſtance which has diſturbed the common order of cryſtallization; and this he propoſes to verify when he can procure a ſufficient quantity of this matter.

C. Sage has deſcribed in a memoir proceſſes by which he was enabled to ſeparate, in the dry way, ſilver from cobalt, and to purify the latter ſubſtance, as well as nickel, in ſuch a manner, that theſe two ſemi-metals, when fuſed into thin plates, could be eaſily magnetized according to Coulomb's method; and that, when ſuſpended by a ſilk thread, they indicated the poles, and exhibited the magnetic phænomena obſerved by Klaproth, Haüy, and ſome other philoſophers.

The ſame chemiſt read another memoir on the alteration produced by light on ſulphurated red arſenic, known under the name *realgar*. He has ſhown that this realgar and *orpiment*, or yellow ore of arſenic, are only the ſame ſubſtance differently coloured; that light changes realgar into orpiment; and that the latter mineral, which paſſes to the ſtate of realgar by the action of fire alone, returns to that of orpiment merely by the contact of light.

Botany.—C. Ventenat has preſented the ſixth and ſeventh number of his *Deſcription des Plantes rares du Jardin de Cels*, &c.; which C. Redouté jun. has enriched with his drawings.

In the ſitting of the National Inſtitute held on the 5th of Germinal (26th March), C. Lalande, having requeſted leave

leave to ſpeak, addreſſed the Inſtitute as follows:—"Citizens colleagues, the Royal Society of London decrees every year a prize, founded by Copley, to the perſon who has written the moſt important work in regard to the ſciences.

"I requeſt permiſſion from the Inſtitute to place in the Mont de Pieté 10,000 francs, the intereſt of which ſhall be employed for giving a gold medal, or the value of it, every year to the perſon who ſhall have made the moſt curious obſervation or written the moſt uſeful memoir relating to aſtronomy in France or elſewhere, whether belonging to the Inſtitute or not, according to a report of commiſſioners whom the Inſtitute ſhall chooſe from the ſection of aſtronomy or the analogous ſections.

"Failing an obſervation or memoir ſufficiently remarkable, the commiſſion ſhall have the right of decreeing the medal, as an encouragement, to any pupil who ſhall have given proofs of his zeal for aſtronomy. They may alſo reſerve it to be doubled the next year.

"If the Inſtitute, before it accepts this foundation, thinks it has need of the permiſſion of government, I beg it may requeſt it. I ſhall be indebted to it for being able to reſtore to aſtronomy a part of what I have received, and of what I have been hitherto able to make."

The Inſtitute received this propoſal with great ſatisfaction, and decreed that commiſſioners ſhould be nominated from the three claſſes to ſee it carried into execution.

We may here add, that there is at the ſecretary's office of the Inſtitute a will, dated February 3, 1768, by which C. Lalande bequeathed to the Academy of Sciences the whole of his property for the purpoſe of giving annual rewards for the improvement of aſtronomy; but at that time there was no aſtronomer in his family.

In the public ſitting of Germinal 15, (April 5th,) C. Lalande read a memoir on the ninth planet.

C. Toulongeon, a memoir on the eſtabliſhment and foundation of new colonies.

C. Champagne, an account of the life and writings of C. Creuzé Latouche.

C. Mongez, a memoir on the agricultural implements of the antients, and particularly their ploughs.

C. Cuvier, an account of the life and works of C. Darcet.

The following prize ſubjects have been propoſed by the Inſtitute:

1ſt, *Ancient Languages.*—A critical examination of the Greek and Latin authors who have written on Egypt ſince the earlieſt ages to the time of the cruſades.

The

The prize will be a gold medal of the weight of five hectogrammes, and will be adjudged in the public ſitting of Niveſe 15, year 12 of the republic. Papers will be received till the 15th of Vendemiaire, year 12.

Hiſtory.—What has been the influence of the reformation of Luther on the political ſituation of the different ſtates of Europe, and the progreſs of knowledge?

The prize will be a medal of the ſame value, and will be adjudged in the public ſitting of Meſſidor 15, year 11. Papers will be received till the 15th of Germinal, year 11.

Chemiſtry.—The Claſs of the Mathematical and Phyſical Sciences had propoſed, in the year 8, the following queſtion:

"What are the characters which in vegetable and animal matters diſtinguiſh thoſe which act as a ferment and thoſe in which they produce fermentation?"

But as the memoirs received did not fulfil the conditions, the Claſs again propoſed it for the year 12.

The prize will be a gold medal of the value of a kilogramme, and will be adjudged in the public ſitting of Germinal 15, year 12.

Painting.—What is the influence of painting on the arts of commercial induſtry? To make known the advantages which the ſtate derives from this influence, and that which it has reaſon to expect?

The prize will be a gold medal of the weight of five hectogrammes, and will be adjudged in the public ſitting of Niveſe 15, year 12.

MUSÆUM OF NATURAL HISTORY, PARIS.

A letter from C. Martin, dated Cayenne, 28th December, contains very ſatisfactory details reſpecting the cultivation of the ſpiceries: he only waits for the favourable ſeaſon to undertake ſome trials in regard to that of the pepper plant. Victor Hugues, agent of government, has aſſigned to him a piece of ground for that purpoſe. The object is to aſcertain what are the trees fitteſt for ſerving as props to the pepper plants, giving preference to thoſe which, being ſuſceptible of multiplication by ſlips, have a thick ſpongy bark, and which riſing to a ſmall height have, however, a long duration. But this is not the only reſult which it is wiſhed to obtain by theſe trials. The iſland of Cayenne is the diſtrict which was firſt cultivated in that colony; the ſoil in ſome parts is exhauſted, and it is neceſſary to leave it at reſt before any other plants can be committed to it with hopes of ſucceſs: beſides this inconvenience, it is expoſed to a ſcourge ſtill more prejudicial to cultivation,—it is ravaged by ants, which devour

devour every thing. There are ſome places where it is impoſſible to prevent the devaſtation occaſioned by theſe inſects, and many diſtricts are abandoned to them. But ſince the introduction of the pepper plant it has been obſerved that they do not ſeem to touch the leaves of that ſhrub. Some plants ſcattered throughout the different eſtates have been ſpared by them. If this fact is confirmed in regard to large plantations, the ſoil of the iſland will be gradually renewed, and its produce will increaſe in the ſame proportion.

C. Martin tried to propagate the female nutmeg-tree by layers. If the operation ſucceeds, it will, no doubt, be the ſpeedieſt and ſureſt means of propagating this ſpicery; for among the nutmegs which are ſown, and which germinate exceedingly well, there is a much greater number of male than of female individuals. He propoſes alſo to graff the latter on the males, and to try for this purpoſe the different proceſſes likely to produce a ſatisfactory reſult.

In regard to the bread-fruit trees, they thrive in an admirable manner. C. Martin announces that he will ſoon have twelve new layers to ſeparate from their parent ſtock. Some of them have already ſuckers, and the reſt will have them ſoon. He has obſerved, that layers in general, if the branches are ſomewhat ſtrong when they are buried, will produce fruits the ſame year.

A letter has been received from C. Reidlé, the gardener who accompanies captain Baudin, dated Iſle de France, April 20, 1801. The following is an extract from it:

"We arrived here on the 14th of February, five months after our departure from Paris. Our paſſage from the iſland of Teneriffe took up more than four. You have no doubt learned the accident which I met with two days before I quitted the iſland. I fell from a rock about 45 feet in height, in conſequence of which I was confined to my bed during three months. I ſtill ſuffered from my wounds on my arrival at the Iſle ofFrance; and I entertained great fear that this misfortune would have prevented me from labouring during the remainder of the voyage. But, thank God, I recovered, and now traverſe the mountains as before. I have already collected 255 ſpecies of plants, without reckoning double ſpecimens for the herbal of the muſæum.

"I have made a catalogue of the colonial garden. I have marked thoſe trees which are found in the Muſæum of Paris, and thoſe which are wanting there: the latter are in number ſixty. C. Ceré has promiſed to give me plants on our return from the South Sea. I have given him in exchange the following trees: two ſree olive, two pears, two apples, a cherry,

an

an apricot, a peach, an almond, a cheſtnut, two wild cheſtnuts, eight ſmall walnuts; making part of the collection of trees entruſted to my care when I left France.

"He made me taſte ſome of the fruit of his bread-fruit trees, which I found delicious. He diſtributed ſome to the principal inhabitants of the colony, to induce them to cultivate them. That which we ate weighed eight pounds. Eight ſtill remain on the trees which have produced. Theſe trees are 18 inches in circumference and 15 or 16 feet in height. They would have been higher had they not been broken at the top by the wind.

"I cannot ſufficiently expreſs the civilities I have experienced from the inhabitants of this iſland. I have viſited their gardens, and every where left ſeeds of the pulſe and flowers of Europe. During our paſſage I planted walnuts, and raiſed a multitude of young plants. I have diſtributed about thirty in the colony, and have ſent ſeveral to the Iſle de la Reunion.

"I cannot inform you whither we ſhall go when we leave the Iſle de France. This day we have received orders to go on board, and we are ready to ſail. But every thing in our expedition has been greatly changed: our touching here has been hurtful to it in more than one reſpect. The ſailors have deſerted to go on board privateers. Some of them have been caught. The captain has landed two ſick officers. Several of the naturaliſts ſeem determined to go no further: as for my part, I ſhall proceed with the veſſel wherever it goes."

By a ſecond letter, dated the next day, C. Reidlé announces that the departure of the veſſel was fixed for the 23d of April: he thinks that they were bound to New Holland.

BRITISH MINERALOGICAL SOCIETY.

In the beginning of the year 1799 a few gentlemen attached to the ſtudies of mineralogy and chemiſtry, with the view of illuſtrating an intereſting department in the natural hiſtory of their native country, which had hitherto been paſſed over with ſlight regard, agreed to form an aſſociation for this purpoſe under the name of the Britiſh Mineralogical Society. They ſaw, with regret, while inſtitutions of this kind were multiplying in the ſtates of Germany and other parts of the continent, that the Britiſh iſlands, among their general and provincial ſocieties, poſſeſſed none whoſe attention was ſpecially directed to this important object. Without being jealous of foreign interference, they were ſorry that Engliſhmen ſhould be almoſt wholly indebted to ſtrangers for an acquaintance with their own mineral treaſures, and that the names

of

of Ferber, Klaproth, Raspe, Jars, and Faujas de St. Fond, should stand the foremost among those who have illustrated the mineralogical geography of Britain. Induced by these sentiments, and conscious that every accession of facts, however small, in so extensive and so little explored a department, was not only of intrinsic value, but might serve as the foundation and commencement of wider investigations, the original members, though few in number, hesitated not to commence a society, whose objects, in their full magnitude, can only be adequately effected by length of time and liberal assistance. They have quietly employed themselves for more than three years in the gratuitous analysis of specimens, in the gradual accession of members both capable and well disposed to exert themselves, to the best of their abilities, in furthering the intentions of the Society; in arranging correspondencies, and forming various internal regulations, which, uninteresting as they may be to the public, are essential to the prosperity of the Society. Having overcome most of the difficulties which at first presented, and having received from various quarters unequivocal assurances of cordial cooperation, the Society feels itself called upon to make a public official statement of its objects, and to communicate from time to time such abstracts of its proceedings as may contribute in any degree to the advancement of science or the arts, and convey to the corresponding members a brief but faithful record of the labours of their associates.

One of the primary objects is to obtain as much information as possible concerning the local or geographical mineralogy of Britain. On this account the Society is strenuously exerting itself to obtain lists of mines, quarries, and mineral springs, from the different counties, together with specimens and such particulars as the proprietors may be willing to communicate. From many of the corresponding and ordinary members, the Society expects also, with confidence, to receive accounts, as accurate as circumstances will allow, of the prevalent rocks and strata in various mineral districts, their extent, direction, and other particulars which may hereafter serve as materials upon which to found a general history of the mineral topography of the island. Among other valuable communications already received for this purpose, a manuscript map of Cornwall deserves especial notice.

A very serious obstacle to mineralogical inquiries is the want of an uniform nomenclature among those who are actively engaged in the working of mines: the same substance is not unfrequently called by different names, or the same name is applied to different substances even within the same

mineral district; the scientific and technical terms are perpetually at variance, and give rise to innumerable errors. A reform in the language of the miners, how desirable soever, is yet wholly impracticable; but the Society expects to render no small service to future mineralogists, by collecting from Cornwall, from Wales, from Derbyshire, and other districts, full and accurate lists of the miner's terms, accompanied by illustrative specimens, the former of which, in due time, will be laid before the public, and the latter will be preserved in the Society's cabinet as authorities, to which a liberal access will at all times be allowed.

When it is recollected how often valuable ores, by appearing under an unusual form, or by presenting difficulties in their analysis that are not to be overcome by the common methods, are neglected and thrown aside; it will not be deemed a trifling object in the Mineralogical Society to undertake the analysis of specimens of this description. All such will be gratuitously analysed, provided the conditions already specified in the circular letters of the Society * are strictly complied with.

The above are the leading views of the British Mineralogical Society, and will always occupy the principal part of its attention. It is by no means, however, intended to neglect other objects: the examination of minerals which have been either not at all, or but imperfectly investigated, will occasionally form a part of its employment, and every occurrence that throws light upon the difficult subject of chemical analysis will be carefully registered in the books of the Society.

LIST OF MEMBERS.

A. Aikin, president.
W. H. Pepys, secretary.
C. R. Aikin, secretary for correspondence.

Members in Ordinary.

W. Allen.
A. Tilloch.
R. Knight.
Th. Cox.
Dr. Babington.
R. Phillips.
R. A. Cox.
P. Sandman.
R. Stocker.
C. Lynam.
R. Bingley.

Corresponding Members.

R. Kirwan, Esq. P. R. I. A. Dublin.
W. Henry, Esq. Manchester.
D. Mushet, Calder Iron-Works, N. B.
L. Dillwyn, Esq. Walthamstow.

* See Philosophical Magazine, vol. vi. p. 369, and vol. ix. p. 282.

Rev.

Rev. W. Turner, Newcaſtle on Tyne.
J. Williams jun. Eſq.
G. Overton, Eſq.
H. Campbell, M. D.
M. Tupper, Eſq.
G. Yeates, M. D. Bedford.
J. Taylor, Eſq. Taviſtock.

XLIX. *Intelligence and Miſcellaneous Articles.*

ASTRONOMY.

Another new Planet diſcovered.

"M. OLBERS, a celebrated aſtronomer of Bremen, writes to C. Burckhardt that he diſcovered on the 28th of March a new comet or planet which reſembles a ſtar of the 7th magnitude. It had 184° 56′ of right aſcenſion, and 11° 53′ of north declination, at 9^h 25′. He obſerved it ſeveral days ſucceſſively, and the 1ſt of April, at 8^h 1′, it had 184° 15′ of right aſcenſion, and 12° 54′ of declination.

"I have learned that prince Henry of Wurtemberg, who reſides at Hamburgh, has purchaſed the beautiful aſtronomical inſtruments made at Paris by Megnié, one of our ableſt artiſts.

"This prince is brother-in-law to the empreſs dowager of Ruſſia; and there is reaſon to hope that the emperor Alexander, whoſe munificence in every thing that relates to the ſciences is well known, will favour the eſtabliſhment of the obſervatory projected by the prince of Wurtemberg, and which is wanting to the large city of Hamburgh.

"LALANDE."

In the above notice, which we have tranſlated from the French official journal (the *Moniteur*), our readers will obſerve announced a diſcovery of Dr. Olbers. On the ſame ſubject we have received the following notice from an able aſtronomer, to whom we have been indebted for ſeveral valuable communications:

"The following are the obſerved places of a new celeſtial body diſcovered by Dr. Olbers, at Bremen, and conjectured to be a new planet:

1802.	R. A.		Declin. N.
March 28,	184° 56′ 49″	-	11° 33′ 30″
29,	184 46 36	-	11 52 59
30,	184 35 32	-	12 15 8.3"

Dr.

Dr. Olbers's planet has, we underſtand, been ſeen in England by Mr. Stephen Lee, of Hackney, and Mr. Aubert, of Highbury, who both agree that it has a greater reſemblance to the old planets than Ceres Ferdinandea.

ANTIQUITIES.

A ſuperb ſtatue of Achilles, nine Roman palms and a half in height, and in perfect preſervation, has been lately dug up in the environs of Oſtea. The hero holds a metal lance in his right hand and the *parazonium* (ſword) in the left: the helmet, which covers his head, has a large plume ſimilar to that of the celebrated ſtatue of the Villa Borgheſe, which is not of ſo excellent workmanſhip. This production, the work of one of the beſt ſculptors Greece ever produced, is not inſcribed with the name of the artiſt. The inſcription *Votiva Marti*, engraved on one of the legs, proves that it was conſecrated to the god of war.

UNCOMMON DISEASE.

The thigh of a woman, who lately died in Geneva, has been ſent to the Medical Society of that city, as exhibiting a very extraordinary phœnomenon. It is ſtuck quite full of ſmall thorns, which the woman, it appears, had been in the habit of ſwallowing, and which gradually making their way through the inteſtinal canal and the blood-veſſels, ultimately lodged in the femoral muſcles. C. Albert is appointed to draw up a report upon the circumſtances of this ſingular caſe.

A caſe ſomewhat ſimilar occurred ſome years ago in the infirmary at Nottingham. A woman was admitted as a patient, from one of whoſe breaſts a number of pins were diſcharged with excruciating pain; and the diſeaſe was by her ignorant neighbours aſcribed to the effects of witchcraft. The breaſt was obliged to be cut off, and the woman recovered perfectly; but, ſtrange to tell, inſtead of being cured of a habit of putting pins in her mouth when undreſſing, and with which ſhe often went to ſleep, in ſome time after, ſhe returned to the infirmary, and was obliged to loſe the other breaſt. We do not recollect whether ſhe recovered after this ſecond operation.

VOLCANIC ERUPTION.

By letters from Banda, one of the Spice iſlands, intelligence has been received at the Hague of a dreadful exploſion of the burning mountain in that iſland. For ſome time before, the air had been humid, accompanied by ſubterranean noiſes; but ſo violent an exploſion has not been known for many years. All the plantations were entirely devaſtated; a great many inhabitants loſt their lives, and the country, to the extent of ſeveral miles, was as inundated by the lava.

LI. *Biographical Account of the late Dr.* PULTENEY. *By* J. AIKIN, *M. D.*

RICHARD PULTENEY, M.D. F.R.S. L. & E. was born in the year 1730 at Loughborough, in Leiceſterſhire. His parents had thirteen children, of whom he alone arrived at the age of maturity. From early youth he was of a delicate habit, and ſuppoſed to be inclined to a conſumption; and it was by means of rigid temperance, which he obſerved during his whole life, that he maintained himſelf in a tolerable ſtate of health. He has recorded (in Mr. Nichols's Hiſtory of Leiceſterſhire) his obligations to his uncle, Mr. George Tomlinſon, of Hathern, who poſſeſſed ſome property in that village, and adorned an obſcure ſtation with virtue and ſcience. "Thoſe (ſays Dr. Pulteney) who remember and intimately knw the ſubject of this memoir, will not, it is believed, judge it otherwiſe than impartial, though, confeſſedly, a tribute from his neareſt relative, one who reveres his memory with the trueſt affection, who, through the early ſtage of life, received from him, as from a father, the genuine dictates of wiſdom, virtue, and religion; all of which were truly exemplified in his own conduct throughout the whole of his life." From this relation he imbibed his taſte for botanical ſtudies; and it was probably through his inſtigation that he was deſtined to the medical profeſſion.

The youth's firſt ſituation in a profeſſional capacity was that of apprentice to an apothecary in Loughborough; an humble ſchool, which, however, his induſtry and talent for obſervation were able to render inſtructive. He paſſed through the uſual courſe of a country education, and then complied with an invitation to ſettle at Leiceſter. That town, like moſt provincial capitals, was divided into two political and religious parties; and it was that of the diſſenters (to which his parents belonged) whence Mr. Pulteney received his ſupport. His ſphere was ſtill further narrowed, by the limitation of practiſing only as an apothecary; for it was thought due to the conſequence of the party, to poſſeſs a ſurgeon of their own as a ſeparate profeſſional character, which office was filled by Mr. Cogan, a man of merit and agreeable manners.

Few remarks can be neceſſary on the hardſhip of placing perſons of abilities and liberal ſentiments in ſituations ſo unfavourable to the acquirement of that reputation and thoſe emoluments

emoluments which are juſtly due to profeſſional ſuperiority; and in which they muſt be reduced to an unworthy and degrading dependence upon a few party-leaders!

Mr. Pulteney was of a timid and cautious diſpoſition; and, though his mind was by no means formed for ſhackles, his temper was not firm enough to enable him effectually to aſſert his freedom. It would be an unpleaſant taſk to dwell upon the ſhare he had in thoſe "ſcorns which patient merit of the unworthy takes;" or of the ſtruggle he maintained with narrow circumſtances, which obliged him to contract habits of rigid œconomy, rendered more neceſſary by the paſſion for buying books, to which he was content to ſacrifice every other inclination. Science was, indeed, his great reſource under the diſcouragements of his ſituation, and it eventually proved the means of raiſing him from obſcurity. To his private friends he was known as one who had inquired largely and thought freely on a variety of topics. To the public he firſt appeared as a votary of the pleaſing ſtudy of botany. He became a correſpondent of the Gentleman's Magazine at an early period; and communicated to it, anonymouſly, a ſeries of valuable letters concerning the poiſonous plants of this country, and a diſſertation on Fungi, contained in the xxvth volume of that miſcellany. To the ſame publication he ſent, in 1757, a tranſlation of a curious paper in the Upſal *Amœnitates Academicæ* on "the Sleep of Plants." This ſubject he purſued more at large in a paper inſerted in the lth volume of the Philoſophical Tranſactions, for 1758, entitled "Obſervations on the Sleep of Plants, with an Enumeration of ſeveral Plants which are ſubject to that Law." He had before appeared among the contributors to the Philoſophical Tranſactions by a "Catalogue of the rare Plants of Leiceſterſhire, with Botanical and Medical Obſervations;" vol. xlix. for 1756. This paper he gave to Mr. Nichols, in an improved ſtate, in 1795, who has inſerted it in the firſt volume of his hiſtory of that county. In 1758 he printed, in the Gentleman's Magazine, a tranſlation from the ſame Amœnitates, of the inſtructive paper entitled "Pan Suecus," giving a catalogue of plants which, from experiment, were found to be either choſen or rejected as food by the different ſpecies of domeſtic quadrupeds. This he adapted more particularly to Engliſh readers by referring to Engliſh authors; and he ſubjoined to it ſome notes and obſervations. Its utility cauſed him afterwards to annex it, in a more enlarged form, to his "View of the Writings of Linnæus."

He diſtinguiſhed himſelf in a manner more purely profeſſional by a paper publiſhed in the Philoſophical Tranſactions,

vol. lii.

vol. lii. for 1761, giving an account of a ſingular medical caſe attended with palpitation of the heart and other uncommon ſymptoms, and which, upon diſſection, exhibited a preternatural enlargement of that organ. In 1762 he received the honour of being elected a fellow of the Royal Society. His name was now aſſociated to thoſe of men of ſcience in various departments; and his perſonal merits were becoming known to a wider circle of acquaintance, to whom he was endeared by his modeſt worth, and the good ſenſe and diſcretion which peculiarly characterized him. Nor can it be doubted, that, even with his original diſadvantages of ſituation, he would have attained a reſpectable ſhare of buſineſs at Leiceſter, though ſtill in that inferior branch of the profeſſion on which he had at firſt entered, to which, however, he had added the practice of midwifery. But it was his lot to poſſeſs a friend whoſe ardent and enterpriſing ſpirit was an admirable corrective of his own diffidence, and who eſteemed him too much to acquieſce in his continuing in a rank and employment beneath his merits. This was Mr. Maxwell Garthſhore, then eminent in medical practice at Uppingham, in Rutlandſhire. By means of a common friend, much revered by both, they were made acquainted in the year 1758, and this acquaintance ſoon ripened into a warmth of friendſhip which death alone could extinguiſh. As it was Mr. Garthſhore's own plan, after a reſidence for ſome years at Uppingham, to take the degree of doctor at Edinburgh, where he had received his medical education, he ſtrongly urged Mr. Pulteney to accompany him thither, and offer himſelf to the examinations of the univerſity, though he had never enjoyed the advantage of academical inſtruction there or elſewhere. His reluctance was at length overcome; and the two friends ſet out upon their expedition in the ſpring of 1764. Mr. Pulteney was already known by reputation at Edinburgh, particularly to Dr. Hope, the profeſſor of botany; and he had the benefit of his companion's extenſive connections in the place. He paſſed through all the neceſſary preliminaries with credit, and in May received the honours of graduation. The ſubject of his inaugural diſſertation was "De Cinchona," or, On the Peruvian Bark; of the natural and medical hiſtory of which important article he gave a very ſatisfactory and inſtructive account. The botanical deſcription is particularly accurate, and is illuſtrated by a plate; and his Theſis has been thought worthy of reprinting in a collection of the moſt valuable compoſitions of the kind which the medical ſchool of Edinburgh has produced.

A circumſtance relative to his graduation, honourable to

himſelf, but affording matter of reflection relative to the conduct of public bodies, ought not to be paſſed over in ſilence. The univerſity of Edinburgh had now for a conſiderable time been riſing in reputation as a ſchool of medicine, and its degrees in that faculty became of courſe more and more reſpectable. It is well known that the univerſities of Scotland, modelled upon thoſe of the continent, have adopted the practice of conferring degrees upon examination, without requiring in the candidates a previous reſidence in their own ſeminary, or, indeed, in any other. In ſome of them the examination itſelf has been diſpenſed with, and the requeſted diſtinction has been beſtowed upon perſons at a diſtance, in conſequence of mere recommendation. It is no wonder that ſuch a laxity ſhould have thrown occaſional diſcredit upon academical honours; nor that the public ſhould have been prone to confound the degrees conferred at univerſities ſimilarly conſtituted, in one general note of diſeſteem. The Edinburgh medical ſtudents juſtly conſidered themſelves entitled to be regarded among thoſe of the profeſſion who had received the greateſt advantages of education, and were the moſt deſerving of thoſe teſtimonials of competency which titular diſtinctions imply. They had therefore begun to remonſtrate againſt a mode of conferring degrees which might confound them with perſons altogether unworthy of the honour; and their diſcontent had been aggravated by ſome late inſtances of notorious incapacity in Edinburgh doctors by favour. Thinking their complaints not ſufficiently attended to, ſome of the ſtudents of the longeſt ſtanding had entered into a mutual engagement publicly to oppoſe every future attempt at decorating with the degree of doctor of phyſic at Edinburgh any perſon who ſhould not have ſtudied there, and to take their own degrees elſewhere in caſe their oppoſition ſhould prove unſucceſsful.

It happened that Mr. Pulteney was the firſt candidate under theſe circumſtances, after this reſolution was adopted. The ſubſcribers handſomely expreſſed to him their concern that a perſon of his acknowledged merit ſhould be the object of their oppoſition; but they adhered to their determination. His reputation and intereſt carried him through the conteſt; but he was (I believe) the laſt in favour of whom the condition of ſtudying at that individual ſeminary has been violated. And ſo ſenſible have the Edinburgh profeſſors ſince become, that augmenting the credit of their univerſity's degrees, and the difficulty of obtaining them, was conducive to their own perſonal emolument, that they have extended the period of requiſite ſtudy there from two to three

years, and made it comprehend every ſet of lectures which can poſſibly be conſtrued as belonging to a complete medical courſe!

As Dr. Pulteney had now aſſumed a new rank in the profeſſion, it was adviſable that he ſhould look out for a new ſituation. The firſt plan which ſuggeſted itſelf to his London friends, was to procure him an introduction to the celebrated earl of Bath, then in a very declining ſtate of health. This was effected; and the earl, upon inſpection of his pedigree, recognized his deſcent from the antient family of which his own was a branch. He alſo, upon converſing with him, was ſo favourably impreſſed with his profeſſional and literary merits, that he reſolved to attach him to himſelf in the character of domeſtic phyſician. He propoſed to ſettle upon him an appointment of 400*l.* per annum; and the connection would probably have been attended with mutual ſatisfaction and advantage, had not the death of the earl followed ſo ſpeedily, that Dr. Pulteney received only one quarterly advance of his intended ſalary.

Not long after this event, a medical vacancy happening at Blandford, in Dorſetſhire, he was urged by Dr. Watſon, Dr. Baker, and others of his friends, to go down and occupy it. Provided with their warm recommendations, but an utter ſtranger to all the inhabitants of the town and its vicinity, he fixed his abode in that ſpot which was to be his reſidence during the whole remainder of his life. A ſmall country town, in the midſt of a neighbourhood compoſed of the uſual ingredients of provincial ſociety, was not, perhaps, exactly the ſituation moſt deſirable to a man whoſe mind was enlarged by free ſpeculation and ſcientific purſuit: but it was now Dr. Pulteney's buſineſs to eſtabliſh himſelf in his profeſſion; and to that object, prudence required that ſacrifices ſhould be made. This is, indeed, the condition of all who have their way to make in the world; and perhaps a juſt ſenſe of true dignity of character, as well as regard to pecuniary advantage, ſhould lead a man to place before him, as his *primary object*, the attainment of ſucceſs in the profeſſion which he has choſen; and to conſider as ſecondary and ſubordinate all reputation or gratification derived from other ſources. Dr. Pulteney, therefore, ſeems to have ſat down with the reſolution, not only of fulfilling his medical duties with the utmoſt punctuality, but of avoiding every thing which might in the leaſt degree involve him in differences with thoſe on whoſe good opinion he was to depend. He was ſenſible that by his removal he had entirely *changed his latitude*; and though he was not a man to ſhift his ſen-

timents and language according to his company, yet he was conſtitutionally cautious, and could, without much effort, practiſe the allowable policy of ſilence. "Commune with thy heart and be ſtill," was the maxim of 36 years of his life. That it exerted its natural influence upon his character, will not be denied; but it did not prevent him from being a very amiable, uſeful, and reſpectable member of ſociety.

The ſituation of Blandford had not hitherto afforded any great ſcope for medical practice; but Dr. Pulteney ſoon extended its limits. His reputation ſpread through the circumjacent country, and he received profeſſional calls from the market and trading towns in a compaſs of twenty or thirty miles round his centre, as well as from many of the country families of principal diſtinction in that part of the kingdom. As his induſtry was great, and his expenſes were moderate, he began to accumulate property. He continued to live in a ſtate of celibacy till October 1779, when he married miſs Elizabeth Galton, of Blandford. He could not have choſen more fortunately for domeſtic happineſs; and the addition this connection made to his comforts was proportionable to the want he had previouſly felt of that ſociety which alone can intereſt the heart. No children were the fruit of this union; but in the additional ſociety of an amiable young relative of Mrs. Pulteney he enjoyed the pleaſure of an adoptive parent.

He continued to employ his leiſure in occaſional writings on topics of medicine and natural hiſtory. In 1772 he addreſſed a letter to his friend Dr. Watſon (publiſhed in the Philoſophical Tranſactions, vol. lxii.) concerning the medicinal effects of the *Œnanthe crocata*, an umbelliferous plant of a poiſonous nature, the juice of which was exhibited, by miſtake, inſtead of that of the water-parſnep. In the lxviiith volume of the ſame collection, for 1778, he gave an accurate account of the bills of mortality for the pariſh of Blandford during forty years paſt, with obſervations. To the London Medical Journal, vol. v. he communicated an account of the poiſonous effects of the Hemlock Dropwort, (the *Œnanthe crocata* above mentioned.)

He had hitherto appeared as an author only in detached memoirs inſerted in periodical publications. But in 1781 he ventured to offer to the public a ſeparate volume, on a ſubject, indeed, with which no man could claim a more intimate acquaintance. This was "A General View of the Writings of Linnæus," 8vo. The purpoſe of this work was to afford an exact ſynopſis of all the labours of the great Swediſh naturaliſt, who appears to have been the object of his

his warmeſt admiration. Along with the account of his works, memoirs of his life are interwoven, chiefly extracted from the different writings of Linnæus. In the prefatory advertiſement Dr. Pulteney ſpeaks with great modeſty of his performance, which, however, was very well received by the friends of natural hiſtory, and obtained for him the preſent of a medal from Stockholm, as an acknowledgment of the juſtice he had done to the fame of the illuſtrious Swede. Many judicious obſervations and valuable points of information are interſperſed in the work. It concludes with a ſynoptical account of all the papers contained in the firſt ſeven volumes of the *Amœnitates Academicæ*.

Some years afterwards a more extenſive and original work proceeded from Dr. Pulteney's pen, and which muſt have coſt him much varied reſearch in its compoſition. This was his "Hiſtorical and Biographical Sketches of the Progreſs of Botany in England, from its Origin to the Introduction of the Linnæan Syſtem;" 2 vols. 8vo. 1790. He paid a juſt tribute to ſcientific merit in dedicating the firſt volume of this performance to Sir Joſeph Banks; and a grateful return to long friendſhip, in inſcribing the ſecond to Sir George Baker and Dr. Garthſhore. The work itſelf is highly valuable, as an example of that union of the hiſtory of men with that of an object of their common purſuit, which is ſo peculiarly intereſting and inſtructive. It has likewiſe made an addition to national biography, which will be duly prized by thoſe who are attached to their country's reputation. It is marked throughout with that candour and diſpoſition to commend which always characterized the amiable author.

Whilſt he was thus tracing the progreſs of his favourite ſcience in books, he was by no means inattentive to the volume of nature as it lay diſplayed before him. The county in which he reſided is conſiderably furniſhed with objects worthy the notice of the naturaliſt, eſpecially in the foſſil kingdom. How well he had made himſelf acquainted with theſe treaſures, the preſent writer obtained a proof, which laid him under a particular obligation. This was a brief but maſterly account of the products of Dorſetſhire, communicated to him for the uſe of his little work entitled "England Delineated." He afterwards enriched the ſecond edition of Mr. Hutchins's "Hiſtory of Dorſetſhire" with a catalogue of the birds, ſhells, and plants obſerved in that county; and during his laſt illneſs he had under reviſal a plate of Dorſetſhire foſſils communicated by himſelf. The formation of a muſæum was the amuſement of many years of his life. By gradual additions, he accumulated a ſtore of natural productions in various

claſſes, which was to him a perpetual ſource of pleaſing contemplation, and will, doubtleſs, become to many ſtudents of nature a means of inſtruction, in the poſſeſſion of the Linnæan Society, to which it was bequeathed.

Dr. Pulteney, in his latter years, frequently expreſſed a wiſh to retire frrom buſineſs, and take up his reſidence in the metropolis, for the ſake of the ſcientific advantages with which it is ſo amply furniſhed; but his habits of life were become too ſtrong to permit him to reſolve upon ſo great a change. He continued, though with diminiſhed ardour, to follow his profeſſional avocations, till he was attacked with a pleuritic complaint, which, after great ſufferings, put a period to his exiſtenee on October 13, 1801, at the age of 71.

By his laſt will he gave a ſignal proof of the deep impreſſion which his early friendſhips had made upon his mind, and which no ſubſequent connections of common acquaintanee could obliterate or equal. After a handſome proviſion for thoſe who on every account were entitled to the firſt place in his remembrance, the remaining objects of his liberality were ſome of the friends of his early days, and even the ſons of thoſe friends. He likewiſe paid a due attention to the claims of charity by bequeſts to the Saliſbury, Leiceſter, and Edinburgh infirmaries, and to the poor of the pariſh of Blandford; and he diſplayed his regard to ſcienee by ſimilar bounties to the Royal Societies of London and Edinburgh, and to the Linnæan Society.

Such are the brief memoirs which I have been able to collect concerning Dr. Pulteney; whoſe life affords, indeed, but little biographical variety, but preſents an encouraging picture of modeſt merit gradually making its way to ſucceſs, and ſcience, even of the moſt retired kind, becoming the paſſport to public eſteem and reputation.

I am happy in being enabled to enrich this article with ſome intereſting anecdotes relative to the early life and ſtudies of its ſubject, obligingly communicated by Dr. Arnold, of Leiceſter, in whoſe words they will be the moſt ſatisfactorily peruſed.

To Dr. Aikin.

Belle Grove, Leiceſter, April 19, 1802.

DEAR SIR,

IT is with pleaſure I ſit down to make you acquainted with ſome particulars relative to the friend and tutor of my youth the late Dr. Pulteney, with whom I had the happineſs and the benefit of enjoying the moſt familiar intercourſe for the ſpace of more than two years and a half, from the beginning of March 1760, to the middle of October 1762; that

that is, from about the 30th to the 32d year of his age; during which time I was a boarder in his houſe, for the ſake of his inſtruction in pharmacy and botany, as a preparation for my medical ſtudies at the univerſity.

He was born at or in the neighbourhood of Loughborough, was much at Hathern, with his uncle Mr. George Tomlinſon of that place; and ſerved an apprenticeſhip at Loughborough to an apothecary, whom I well knew in my youth, of the name of Harris; a man of ſome humour, but of ſmall abilities in his profeſſion, from whom he derived little information but in the common proceſſes of pharmacy: for every thing more than this, he was indebted to his own genius and induſtry.

A taſte for botany he imbibed almoſt in his infancy, from his uncle Tomlinſon, who was a botaniſt of the old ſchool.

His firſt attention to this purſuit was merely the effect of imitation. Seeing his uncle ſearching for plants, as they walked together in the fields; ſeeing him take them home, examine them by the deſcriptions of Gerard and Ray, at that time the ſole, or at leaſt the beſt, and certainly the only good guides in Engliſh botany, and compare them with the rude wooden cuts of the former; diſplay ſome of them upon paper, preſs and dry them, and fit them for a place in his *Hortus Siccus*; he began to do the ſame, to aſk his uncle the names of plants, to make imitative collections, to draw up infantile catalogues and deſcriptions of the plants which he found within the circle of his botanical excurſions and reſearches; and ſometimes he added figures of ſuch as he admired for their beauty, or eſteemed for their rarity.

How early he was a botaniſt appears from what he ſays under the article *Campanula patula* in his "Catalogue of ſome of the more rare Plants found in the Neighbourhood of Leiceſter, Loughborough, and in Charley Foreſt," inſerted in the introductory part of the firſt volume of Mr. Nichols's Hiſtory of Leiceſterſhire, at p. clxxvii., where he has this note:—"In the drier parts of Buddon Wood, and the hedges and lanes adjoining. Firſt diſcovered in England by Mr. Brewer, in 1726, near Worceſter, as recorded by Dr. Dillenius. Next in this place (Buddon Wood) by the writer of this catalogue, in 1742, who a few years afterwards communicated the ſeeds to the gardens of Chelſea and the Britiſh Muſæum *."

Of

* Let me here obſerve, that I have found the *Campanula patula* between the fourth and fifth mileſtones, under the hedge, on the left ſide of the old carrier's road from Birmingham to Coventry; and have obſerved it there,

Of his youthful catalogues above mentioned I have ſeen and examined ſeveral, with deſcriptions and coloured drawings of the moſt remarkable plants, in very ſmall books. They were drawn up at different times, and, like the different editions of a book, were progreſſively improved and enlarged as his experience in botany and his knowledge of the plants in the neighbourhood were improved and enlarged. Some of them were, I believe, made as early as when he was only eight or nine years of age; and I particularly remarked that of the *Campanula patula*, which, as we have ſeen, had not been many years known to be an indigenous plant of this iſland, and which he himſelf had diſcovered in and near Buddon Wood in the year 1742, that is, when he was only twelve years old: there were repeated coloured drawings in the moſt recent of them; done, indeed, in ſuch a ſtyle of mediocrity as might be expected from a boy of that age, but affording a very good likeneſs of the plant which they were intended to repreſent.

This taſte, once implanted in his mind, grew and flouriſhed, and was ultimately productive of extraordinary improvement in the ſame way. Imitation gave a turn to his purſuits; and excellent abilities, united with indefatigable diligence, led him on to that perfection in both the ſcientifical and practical knowledge of botany, a ſtudy at that time but rarely cultivated, to which he afterwards attained, and which at length raiſed him high in the eſtimation, and drew to him the frequent correſpondence, of the moſt eminent botaniſts of the time.

He was early in the habit of examining plants in their ſeveral native places of growth, of being moſt exact in the inveſtigation of them, and in noting their minuteſt differ-

there, invariably, for about twenty years; where ten or a dozen plants are annually to be found, in the month of July, exactly about the ſame ſpot; from whence, ſome years ago, I collected ſeeds, which I ſowed in my garden, of which this beautiful plant forms a conſiderable ornament every year from the beginning of July to the latter end of October. I have alſo found it in the road from Birmingham to Sutton Coldfield; and about a mile from Stourbridge, in Worceſterſhire, where, beſides the common purple ſort, I ſaw one or two plants of a beautiful ſnowy white. This plant is not only rarely to be met with; but I never found it growing plentifully but in Buddon Wood. In general, it is ſeen very thinly ſcattered within a ſmall circumſcribed ſpot.

It is to be obſerved that Buddon Wood is not a great way from Hathern, where he firſt imbibed a taſte for botany; or from Loughborough, where he ſerved his apprenticeſhip; and that none of theſe places are far remote from Charley Foreſt, where many of the rare plants were collected; and that at Leiceſter he was for ſeveral years ſettled as an apothecary, where he remained till his thirty-ſecond year; all which places were the chief ſcenes of his early or more mature herborizations, and furniſhed plants for his catalogue.

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ences and varieties; in drawing up lists of those which he had investigated; in making memorandums of the places in which he had found those which were less common, and of the natural habitations of all; and drew up, occasionally, botanical tables, as well illustrative of the Linnæan system, as of other matters interesting to the accurate botanical student.

But while he attended so diligently to the improvement of his knowledge in botany, he was not less diligent in the study of the theory and practice of medicine, of chemistry, and of every branch of science which could contribute to the advancement of his knowledge of that profession to which he had dedicated the most serious business of his life, and in which his success was no less remarkable than in his botanical pursuits.

His most favourite medical writer was the celebrated Frederic Hoffman, whose voluminous works he read over and over with indefatigable attention and perseverance. Indeed, in whatever study he was engaged, his diligence was intense. I have often known him to sit, when the engagements of his profession did not call him from his studies, and have often sat with him, for whole days together, without interruption, but by the necessary interposition of meals, to which, on such occasions, we usually turned with reluctance. Sometimes, when he was very intent upon a subject, he had a habit, not to be commended, of overspreading his pillows with books, of reading, and referring from one to another; in short, of studying in bed till very late, sometimes till long after midnight.

These habits of close application, both by day and night, served to render more delicate an originally weak constitution; and might have been seriously pernicious in their consequences had they not been frequently interrupted, and sometimes for long intervals, by the necessary business of his profession, which, though not great, was not inconsiderable, and by long botanical excursions, which we used to make on foot, once or twice a week in the summer months, and not rarely in those of spring and autumn.

He took great delight in the history of science and literature, and of scientific and literary men; in which he was a most diligent inquirer, and had collected a stock of information, both published and original, of which few men are possessed. But nothing seemed to give him such exquisite pleasure, or so to excite in him the ardour of enthusiasm, as the lives and travels of naturalists; and especially the herborizing adventures, the great fatigues, the hair-breadth escapes, the discouraging

diſcouraging difficulties, and the happy ſucceſs, ſo common and ſo intereſting in the narratives and hiſtories of the botaniſt who engages in travels of diſcovery.

He read the modern Latin books of medicine and natural hiſtory with great facility; and was particularly converſant with the works of Hoffman, Van Swieten, Ray, Rumphius, Van Royen, Linnæus, and of ſome other eminent medical and botanical writers: but had little ſtudied, and read with difficulty, the claſſical Roman writers of antiquity; and thoſe of Greece not at all. In the French language he was completely verſed, and read a good deal.

The acquiſition of a new book of merit gave him the moſt ſenſible pleaſure. The joy which he expreſſed on the firſt arrival of the ſecond volume of the ſo greatly enlarged tenth edition of Linnæus's *Syſtema Naturæ*, which was publiſhed at Stockholm in 1759, of which I was a witneſs, I ſhall never forget. He ſeized upon it with eagerneſs, ran over it with avidity, and could ſcarcely quit it till he had drawn up an analyſis of the whole, which, I think, he ſent to the Gentleman's Magazine, as he had before ſent an analyſis of the firſt volume to the ſame valuable repoſitory.

His conſtitution was from his birth ſo delicate that his parents found difficulty in rearing him; and experienced great anxiety about him, as he was an only child. At the time when I was in his family he had frequent illneſſes, very commonly from fatigue, eſpecially from riding on horſeback, which always, even after ſhort journeys of perhaps ten or twelve miles, very much diſcompoſed him, and after long rides brought on a conſiderable degree of fever. He had frequent ulcerous ſore throats of a very uncommon kind; being accompanied with great debility, but very little fever; ſcarcely any ſwelling, and no great inflammation of the throat, tonſils, or uvula; but large ſloughs, commencing and running from theſe parts, which were very rapid in their progreſs, but ſoon yielded to the uſe of the Peruvian bark and proper gargles, without producing any alarming ſymptoms or great ſuffering. I was then very young in the knowledge of phyſic; but theſe ulcerous, ſpreading ſloughs appeared to me, as they did to him, very ſingular; and I have never met with any thing like them ſince.

Great as was his attention to medicine and natural hiſtory, his reading was not confined to books in theſe branches of knowledge only. He was well read in hiſtory, morals, and the philoſophy of the human mind; to the latter of which he paid particular application; and, while he ſtudied with diligence the appearances of external nature, and the ſtructure

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and offices of the human body, did not overlook the operations and mechaniſm (if I may uſe ſo bold a metaphor) of the human mind; on which I have often heard him diſcourſe, not only with energy and eloquence, but with great profundity of argument and accuracy of diſcrimination.

Among his early correſpondents were the celebrated Dr. Hill, Mr. Hudſon, the author of the *Flora Anglica*, and Dr. Watſon, F. R. S., beſides others both regular and occaſional. And among the early reſpectable friends whom his medical merit had acquired, were Sir George Baker and Dr. Garthſhore, who then reſided in this neighbourhood; the latter at Uppingham, as you well know, and the former at Stamford. This acquaintance and correſpondence with Dr. Watſon began before or about the time of his firſt entrance upon buſineſs. Having long been in the habit of reading the papers of that eminent botaniſt in the Philoſophical Tranſactions, in the xliid volume of which, No. 471, for November and December 1743, his firſt botanical paper was publiſhed, being an "Account of Dr. Haller's *Enumeratio Stirpium Helveticæ*, extracted and tranſlated from the Latin," &c., and to which he had continued to contribute frequent botanical as well as electrical and medical papers; and having, therefore, long admired him as an experienced and firſt-rate botaniſt, he conceived a ſtrong deſire of cultivating his acquaintance, took opportunities of ſending him botanical communications by letter; and at length, having occaſion to make a journey to London, introduced himſelf to him perſonally; and, being kindly received, from that time a regular correſpondence and friendſhip commenced between them, which did not ceaſe till the death of Dr. Watſon. This intimacy, I have good reaſon to believe, commenced ſoon after he had finiſhed his apprenticeſhip; and not only opened to him the moſt unreſerved intercourſe with Dr. Watſon whenever he had occaſion to viſit London, but was the means of his introduction to the acquaintance of many other men of eminence, as philoſophers and naturaliſts.

At the time when I was his pupil, his *Hortus Siccus* of Britiſh plants was very large; and was particularly copious in the order of graſſes, in the collection and inveſtigation of which he had been ſingularly indefatigable.

He not only correſponded with, but was viſited by, the moſt eminent philoſophers and naturaliſts. Among thoſe who viſited him while I was an inmate in his houſe was the earl of Macclesfield, then, I think, preſident of the Royal Society, who, I remember, was uncommonly pleaſed with his large and beautiful collection of Engliſh graſſes.

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I have now mentioned the moſt material circumſtances which have occurred to me concerning our late reſpected friend. There are ſeveral of them which I have related rather becauſe they were facts, (and every fact relative to a departed friend ſeems intereſting to his ſurvivors,) than becauſe I thought them of ſufficient importance to be laid before the public. I am, dear Sir,

With great eſteem,
Yours very ſincerely,
THOMAS ARNOLD.

LII. *Analyſis of the Arſeniates of Copper and of Iron.* By RICHARD CHENEVIX, *Eſq. F.R.S. M.R.I.A.*

[Concluded from p. 229.]

SECTION III.

Analyſis of the Red Octaedral Copper Ore, in which the Metal exiſts in a State hitherto unknown in Nature.

IN the courſe of the experiments which have been ſtated in the preceding ſections, I have had occaſion to examine a great number of copper ores, and particularly of copper ores from Cornwall; but the only one which has afforded any intereſting reſults, is the well known ſpecies called red copper ore, cryſtallized in regular and brilliant octaedrons. It has been ſo long known, and ſo often mentioned by mineralogiſts, that it may excite our wonder when we reflect, that its chemical nature has never been aſcertained. For it would be an injuſtice to that very accurate and ſcrupulous analyſt M. Vauquelin, to ſuppoſe that he meant to pronounce decidedly upon that point by the ſingle experiment which he had made *, and which is mentioned by the abbé Hauy in a ſhort extract of his cryſtallographical arrangement of mineral ſubſtances, publiſhed in the *Journal des Mines*.

Romé de Liſle, the baron de Born, Lametherie, the abbé Hauy, and indeed every other mineralogiſt, concur in calling this ſubſtance red calx of copper; but ſome of them aſſert that it contains a portion of carbonic acid. Among the many analyſes which have been made of this ore by Fontana, Monnet, De Born, Renovantz, and others, I could not find one, that in the proportions, or even in the ingredients, reſembled what I had found to be its contents. The

* He merely poured muriatic acid upon the ore; and, as it was entirely diſſolved, without effervеſcence, concluded it to be an oxide, and not a carbonate, of copper.

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higheſt amount of copper (that given by Fontana) does not exceed 66 per cent., and is far ſhort of the real quantity. The remainder, as he ſtates, conſiſts of water and of pure and fixed airs. The difference in the reſults I had obtained, together with ſome new facts, which I had occaſion to obſerve during my experiments, induces me to treat the ſubject at ſome length; referring for its external characters to thoſe mineralogiſts above mentioned, who have amply deſcribed the ore, and confining myſelf entirely to its chemical analyſis, and ſome analogous experiments.

One hundred parts of very pure and regularly cryſtallized red copper ore were reduced to a fine powder, and diſſolved, without the aſſiſtance of heat, in nitric acid.

During the operation, a very violent effervеſcence, accompanied by a diſengagement of nitrous gas, unuſually copious and rapid, took place. When theſe phænomena had ſubſided, the ſolution was blue, like every other nitrate of copper; and the ore had entirely diſappeared. The liquor, perfectly limpid, was evaporated to dryneſs; muriatic acid was poured in, and the nitric acid was expelled by a ſecond evaporation. Into the muriate of copper, which remained behind, a plate of poliſhed iron was immerſed, which, after the uſual phænomena, gave a precipitate that was found, upon examination, to be copper, and amounted to 88,5. In order to complete the hundred parts, it would be neceſſary to add 11,5. But fire expelled from the ore neither water nor any other volatile ſubſtance; nor did the weight of a given quantity appear either to diminiſh or to increaſe by long expoſure to a moderately elevated temperature. The only oxide of copper with which I was acquainted, as exiſting in nature, contains 20 per cent. of oxygen. I had therefore 8,5 of copper exceeding the quantity I ſhould have obtained had the ore been wholly compoſed of black oxide of copper. And, on the other hand, as I had convinced myſelf that no loſs of weight had been occaſioned by any part of the metal remaining unprecipitated by the iron from its ſolution, I could not conclude the ore to be in the ſtate of native copper. I was led, therefore, to imagine, that it might be a mixture of thoſe two ſubſtances; and that muriatic acid, by diſſolving the one, and leaving the other untouched, would be the moſt effectual means of producing the ſeparation I deſired, and of determining the proportion of each.

Upon 100 parts of the ore a ſufficient quantity of ſtrong muriatic acid was poured. A total ſolution was effected, accompanied with diſengagement of caloric. The liquor was, at firſt, of a very deep brown, approaching ſomewhat to the

tinge which water will receive when ſtrongly impregnated with the colouring matter of dried vegetable ſubſtances; but, upon being expoſed to the air, and boiled ſome time, it became like every other muriate of copper; and a plate of poliſhed iron precipitated 88 of metallic copper. From this laſt experiment it was evident that no metallic copper was contained in the ore. But ſtill the deficit to be ſupplied by oxygen amounted to no more than 12; while the copious diſengagement of nitrous gas, in the firſt experiment, indicated that the metal was not at its maximum of oxidation; and the rapidity with which it ſeized upon an addition of oxygen, ſufficiently ſhowed how ſtrong was the affinity of that principle for copper, in that particular ſtate in which it exiſts in the ore.

I imagined it would be expedient to attempt ſome precipitations by other reagents, and make ſome further experiments. For this purpoſe, I diſſolved ſome more of the ore in ſtrong muriatic acid; and, when I thought that the acid had taken up as much as it could contain, and that the colour had arrived at its deepeſt tinge, I gently drew off the clear liquor, uſing all the precaution which the nature of the experiment allowed, to preſerve it from the contact of the atmoſphere, and proceeded to examine it. Knowing this ſolution of muriate of copper to be very concentrate, I attempted to dilute it; but what was my ſurpriſe, when, upon the firſt affuſion of water, I ſaw the liquor become turbid and milky, and a very abundant heavy precipitate, of a white colour, fall to the bottom!

Struck with the novelty of this appearance, I proceeded to collect as much of the ſubſtance as I could, in order to give it a thorough examination. For this purpoſe, I decanted the ſupernatant liquor, and continued to waſh the precipitate. Upon every ſubſequent addition of water, I perceived that the precipitate loſt a little of its whiteneſs, and drew towards an orange colour, not unlike the precipitates of platina. I ſoon found, therefore, that by this method I had no chance of obtaining, in a permanent and conſtant ſtate, this muriate of copper, fit to be ſubjected to experiments proper to determine its internal nature and proportions. I then attempted to make uſe of alcohol, as precipitant, inſtead of water; but I found the ſalt to be ſoluble in it, when the exceſs of acid neceſſary for its ſolution in water was preſent. Nor was I more ſucceſsful, when, after having precipitated by water, I waſhed with alcohol; for the colour of the ſalt paſſed gradually from very white to a ſhade of orange; leſs rapidly, it is true, in this caſe, but ſtill ſo as to convince me that I could

could not even thus procure, in a ſtate conſtantly ſimilar, the ſalt I wiſhed to examine. The only concluſion which all theſe experiments entitled me to draw, was that, in the firſt inſtance, water precipitated the muriate of this particular oxide of copper from its ſolution, but in a manner very different from that in which muriate of antimony, of biſmuth, and ſome other metallic ſalts are acted upon. When into either of theſe muriates water is poured, a precipitate enſues, but it retains a very ſmall portion of acid, if any; whereas, in the caſe before us, it is a ſalt, and not an oxide of copper, that is thrown down. In order to effect in this ſalt a decompoſition ſimilar to that which takes place in muriate of biſmuth, or of antimony, it is neceſſary to draw off the firſt liquor, and then proceed to waſh copiouſly. The precipitate will by degrees aſſume an orange colour, which, as we ſhall preſently ſee, is the real appropriate colour of this oxide of copper, prepared in the humid way.

It is evident alſo, from this precipitation, that this oxide of copper combines with muriatic acid by a very ſlender affinity.

As it did not appear to me, that I ſhould obtain any thing very ſatisfactory from this combination with muriatic acid, I reſolved to try ſome other acids. Sulphuric, phoſphoric, oxalic, citric, acetic, tartareous, and acetous acids were each poured upon known quantities of the ore, and kept in bottles completely filled and well ſtopped, in order to prevent any abſorption of atmoſpheric oxygen. The liquors generally became blue; and, upon trial, were found to contain the common and well known ſalts of copper, compoſed of the reſpective acid, and the oxide of copper containing 20 per cent. of oxygen; while a large portion of the ore appeared to remain in its original ſtate. But, as I was certain that there could be no decompoſition in moſt of theſe acids, under the above circumſtances, and moreover, that no oxygen could be taken in from the atmoſphere, it became a matter of no ſmall intereſt to examine from what ſource the metal diſſolved had acquired the neceſſary quantity of oxygen to favour its ſolution, and afford the uſual ſalt of copper, in which it is oxidated in the proportion of 20 per cent.

I repeated, with all the above acids, the experiments tending to ſatisfy that inquiry; but, as the reſults from all were nearly ſimilar, I ſhall mention that only which proved to be the moſt ample and the moſt concluſive.

One hundred parts of the pulverized ore were introduced into a ſmall phial, and dilute phoſphoric acid was poured in, ſo as to fill it. A ground-ſtopper cloſed it completely; and

in that ſtate it was ſuffered to remain three days, during which time the bottle was frequently ſhaken. The acid became at firſt of a light blue, and increaſed in colour by remaining upon the ore. At the expiration of the above term, the liquor was decanted; the reſiduum was well waſhed and dried, and weighed 42. The blue liquor contained merely common phoſphate of copper, held in ſolution by an exceſs of acid. Upon the 42 parts of reſiduum, ſtrong muriatic acid was poured, which did not appear to produce the ſmalleſt change or effect. It was evident, therefore, that ſome previous alteration had been produced; for, if it had remained in its original ſtate, muriatic acid would have acted upon it, as in the caſe already mentioned. To operate more effectually, nitric acid was added, and the whole gently heated. A complete ſolution followed, during which much nitrous gas was diſengaged. The remainder of the nitric acid was expelled by evaporation; and a plate of poliſhed iron, immerſed in this muriate of copper, afforded a precipitate of metallic copper, weighing within one part as much as the weight of the firſt reſiduum. It was evident, therefore, that a partial reduction of the ore had taken place; and, what is ſtill more ſtrange, had taken place by means of the preſence of an acid.

In many obſervations which have preſented themſelves in the courſe of various analytic experiments, ſomething ſimilar had before occurred to me. I have known metallic oxides yield a part of their oxygen, one to the other, in favour of ſome particular ſolvent. When the metallic oxide *A*, for inſtance, containing 25 per cent. of oxygen, is in contact with the metallic oxide *B*, containing 10 per cent. they will each remain quieſcent in their reſpective ſtates. But, if the ſolvent *C* comes to be added, and if the ſubſtance *B*, at 10 per cent. of oxygen, has no affinity for *C*, but at 15 or 20 per cent. has a very powerful affinity for it, then may the oxide *A* lend a part of its oxygen, in order to favour the combination of *B*, at 15 or 20 per cent. with the ſolvent *C*. Indeed, as ſoon as I ſaw the phoſphoric acid aſſume gradually a blue tinge, and the undiſſolved powder begin to wear a more brilliant appearance, I imagined I ſhould not fail to recognize the ſame fact in this caſe. When phoſphoric acid has remained long enough upon the pulverized ore to diſſolve all it can, the oxygen is concentrated, as it were, to the amount of 20 per cent. in the part which is diſſolved; and all that which could not be diſſolved has (through the twofold affinity of copper for oxygen, to the amount of 20 per cent. and of phoſphoric acid for that oxide of copper, at that degree

degree of oxidation,) yielded up its entire ſhare of oxygen, to favour the combinations which take place in a new order, the only one which can exiſt among the ſubſtances now preſent. It is, therefore, to the diſpoſing affinity *, cauſed by the preſence of the phoſphoric acid, which ſeeks to combine with black oxide of copper, that the reduction of 42 per cent. of this ore is entirely to be attributed. All the acids above mentioned are capable of producing the ſame change, but in a manner, perhaps, not quite ſo diſtinct or ſatisfactory.

From the foregoing experiments it appears, that copper exiſts in this ore in a ſtate hitherto unknown in nature; and that it contains much leſs oxygen than has ever been ſuſpected in any oxide of copper; for, from the quantity which was precipitated in the metallic ſtate by iron, it appears to be combined in the proportion of about 11,5 per cent. To confirm this idea, and aſcertain, as nearly as I could, the preciſe quantity, I diſſolved 100 parts in nitric acid; then boiled with potaſh, and filtered. One hundred and eleven remained upon the filter, which, as they had combined with a new portion of oxygen from the nitric acid, were in the ſtate of black oxide, and correſpond exactly to 88,75; ſo that I believe I ſhall be within one per cent. of the truth, in aſſerting the proportions to be,

Copper	-	-	88,5
Oxygen	-	-	11,5
			100,0

When, into a ſolution of muriate of ſuboxide of copper, liquid potaſh or ſoda is poured, a bright yellow precipitate, not unlike the precipitate of platina, takes place. This precipitate differs only in colour from the original ore; for it is ſoluble in muriatic acid, and affords the ſame ſolution and precipitation by water, and the ſame appearance with alcohol. It is likewiſe ſoluble in nitric acid, but with diſengagement of nitrous gas, and gives the ſame appearances with the other acids above enumerated. The difference of colour ſeems to ariſe merely from the tenuity of its molecules, compared to the mechanical pulverization of the natural oxide. When alone and dry, it is much more permanent in its nature than when combined with muriatic acid; but any part

* As the term *prediſpoſing affinity* has been objected to, I have uſed the term *diſpoſing*, which, I truſt, will not be thought improper. When in two bodies which, while together, remain in their original ſtate, the equilibrium of their principles comes to be broken by the preſence of a third, we cannot but allow that it is this third which has diſpoſed them to the rupture of that equilibrium; and, moſt certainly, be the fact explained as it may, whatever diſpoſes may be called diſpoſing.

of it that happens to be in contact with a filter, becomes green, and then blackiſh, leaving a mark of the ſame ſhade upon the paper. Were it not for this property of changing, it might be of uſe in the art of painting; for the colour is extremely beautiful, and would be highly valuable if durable.

The precipitate cauſed in the muriate of copper by the carbonates of potaſh and ſoda, is of a brighter yellow, and is a real carbonate of ſuboxide of copper. But, if ammonia is poured, at firſt in a ſmall quantity, into the above ſolution, the precipitate is blue; and, upon adding an exceſs of the precipitant, the whole is rediſſolved, and the liquor is like any other ammoniacal ſolution of copper.

In order, however, to determine in what ſtate the copper was diſſolved by that alkali, I poured ſome ammonia upon 100 parts of this ſuboxide in a well-cloſed phial. The liquor became blue; and I expected to find that part of the ore had been reduced, as with phoſphoric acid; but the reſiduum was entirely ſoluble in muriatic acid, with the uſual phænomena.

A ſpirituous tincture of galls, poured into muriate of ſuboxide of copper, afforded no precipitate, owing, I ſuppoſe, to the exceſs of acid; but ſulphurated hydrogen gas threw down a black, and pruſſiate of ammonia a lightiſh brown, precipitate.

I endeavoured to obtain muriate of ſuboxide of copper by evaporation, and by diſtillation in a retort; but, as I could perceive the liquor conſtantly aſſume a blueiſh tinge, I could not reckon upon the purity of the ſalt, ſufficiently to ſubmit it to analyſis.

Such were the principal experiments, which the fleeting and precarious exiſtence of the ſalt allowed me to make upon it. But, from ſome properties which I had remarked, I could perceive that this ore was a natural oxide of copper, nearly in the ſame ſtate as that artificial oxide which Mr. Prouſt had found in the white muriate of copper, obtained by pouring a recent ſolution of muriate of tin into a ſolution of muriate of copper.

If, however, by the very nature of the ſubſtance, (which, as I ſaw it ever changing, I thought it would be loſs of time to examine further,) I have been turned aſide from more certain reſults, I have been more ſucceſsful in imitating by art the ſtate of this natural product.

By expoſing oxide, hydrate, or carbonate of copper, without addition, to a violent heat, in an open crucible, I frequently obtained the ſuboxide, which then preſented all the properties already recognized in the above ſpecies of copper ore. In one inſtance, I ſo far ſucceeded, that, upon the very firſt

inſpection,

inſpection, the well-experienced eye of the Count de Bournon recognized a lump of it to be a maſs of ſemi-fuſed, artificial, red copper ore.

But I have found a method of producing at pleaſure, in the humid way, all the new ſalts, and the oxide above deſcribed. As I had found about 11,5 per cent. of oxygen to be the quantity contained in the ore, I took that quantity of black oxide of copper which correſponded to 11,5 of oxygen; (57,5 of black oxide was the proportion thus indicated;) on the other hand, I took 50 parts of metallic copper, which had been precipitated by iron from muriate of copper, and which was in a ſtate of tenuity not inferior to the fineſt powder. Theſe were well mixed, by trituration in a mortar, and put, with muriatic acid, into a well ſtopped phial. A violent diſengagement of caloric took place; the liquor became of the ſame colour as with the ore, and contained a ſalt in every reſpect ſimilar to that afforded by the ore; while a portion of metallic copper remained, with all its luſtre, at the bottom of the phial. The ſolution was decanted, and the reſiduum of metallic copper weighed 7,5. Conſequently, 42,5 had been diſſolved, which, with 57,5 of black oxide, complete the hundred parts.

No experiment could prove, in a manner more ſatisfactory, the quantity of oxygen contained in 100 parts of this ſub-oxide; nor could any afford reſults more important, or more concluſive. The paſſage of a portion of oxygen from one part of the metal to another, to favour its ſolution, as already ſtated, is proved beyond the poſſibility of doubt; and is doubly intereſting, as it is the inverſe of what happens to the ore when treated by phoſphoric acid.

In the experiments of Mr. Prouſt, he has eſtimated the quantity of oxygen, contained in 100 parts of this oxide, to be 17. This proportion was calculated upon the deficit of a ſingle analytic experiment, made upon the ſalt of muriate of ſub-oxide of copper, after having determined the quantity of acid, of water, and of metallic copper. But, firſt, the ſalt cannot eaſily be obtained (as I have before obſerved) in a ſtate ſufficiently certain to be relied on, in an experiment of this nature; and, in the next place, it is probable, as happens in almoſt every analyſis, that the deficit was greater than the real quantity of oxygen. For, the agreement between the analytic and ſynthetic experiments I have juſt ſtated, ſeems to confirm 11,5 to be more exactly the proportion.

When, in the dry way, the above proportions of metallic copper and of black oxide of copper, or, when metallic copper with a correſponding proportion of hydrate or of carbon-

ate of copper, were intimately mixed, and heated at a low red heat, the oxygen ſeemed to be equally diſtributed through all the maſs; and every particle ſeemed perfectly homogeneous.

In the ore here ſpoken of, it is by no means rare to find large pieces of real native copper; and, whether we conſider it mineralogically or chemically, it certainly is an intereſting ſubſtance. But, how much more will it be eſteemed, if it is regarded with a view to public utility!

The Baron de Born has mentioned a gray cupreous pyrites, which, he ſays, contains 90 per cent. of copper. I have analyſed a ſimilar one from Cornwall, (gray vitreous copper ore, p. 212,) which I found to contain 86 of the ſame metal. But if we reflect, not ſo much on the quantity as upon the extreme purity of this copper, and the wonderful facility with which this uſeful metal may be extracted, it will be found much ſuperior to every copper ore hitherto diſcovered. It would be well worth the attention of miners, to keep a conſtant look-out for this ſubſtance, which, I am informed, is not rare in Cornwall. It contains no iron, and no ſulphur; the abſence of which latter is a peculiar advantage. It is a fact not generally known, I believe, that there is hardly ſuch a thing in commerce, as copper which does not contain a little ſulphur; at leaſt, I have rarely met with any ſuch; and it requires but a very minute portion of ſulphur to increaſe the fuſibility of copper. The advantage of obtaining copper free from ſulphur, is too obvious to require to be pointed out; and that advantage does this ore poſſeſs.

To work it ſeparately, if ever it ſhould be found in ſufficient quantity, would well repay the labour it would coſt; and a very ſmall mixture of any diſoxidating ſubſtance would, in a ſhort time, reduce immenſe quantities.

From the foregoing experiments we may perceive into how many errors we may be drawn, if, in arguing from the reſults which we obtain, we pronounce too haſtily upon the ſtate in which a ſubſtance exiſts, in the ſubject of any analyſis. After what has been ſhown, with regard to the action of muriatic acid upon a mixture of metallic copper and black oxide of copper, both reduced to powder, and of the action of phoſphoric acid upon the ore itſelf, it may be ſtill a doubt whether this ore is really a ſuboxide, or a mixture of metallic copper and oxide of copper, at 20 per cent. of oxygen. But, as ſimilar proportions of both, after having been made red-hot, preſented all the properties and appearances of the ore much more ſtrongly than when ſimply mixed, it is fair to conclude that it is a real ſuboxide. Had not muriatic acid been

been used, the natural conclusion would have been, that the ore was a mixture, or at most a combination, of these two substances; for such did it appear to be by the testimony of the other acids. The truth is, we are but little acquainted with the exact state in which substances exist in many natural combinations. However, in the mineral kingdom, such fallacious conclusions are less frequently to be dreaded than in the vegetable and animal kingdoms. But, in every research, it is important to leave as little room for them as possible; and he who would indicate a sure and constant method of ascertaining whether, in many cases, what we deem a component part, is not, in fact, a product of the operation, would render to science a service, the real value of which is, perhaps, not now entirely foreseen.

LIII. *Communications on the Subject of Navigation, from* JOHN COOKE, *Esq. M. R. I. A.*

SIR, Dublin, April 25, 1802.

I REQUEST permission to communicate to the public, through your Magazine, the inclosed attempts to advance the art of navigation; they consist of methods of measuring distance at sea, and of discovering currents, with the plan of a new sea chart. My object in publishing them is to offer them to the consideration of others who are more capable of detecting their imperfections and of suggesting remedies than myself, for which I shall be thankful; but, as I intend to make experiments, and to follow up these inventions, I do not wish to have it understood that I relinquish my exclusive right to them by this step, or that any person may make use of them without my consent. I am, Sir,

Your most obedient servant,

To Mr. Tilloch. JOHN COOKE.

THE distance through which a ship sails is the product of the time and velocity of its progress: the time is easily ascertained by well-known methods, and the log-line exhibits the degree of motion during the experiment with tolerable certainty: but, since the motion is perpetually varying, this method would require an uninterrupted succession of experiments to afford an accurate measure; therefore it is a desideratum in navigation to obtain some easy method of registering the several changes of velocity which take place in sailing, with the intervals of time between each; which objects,

with a practical contrivance for finding the diſtance at any time by theſe data, are aimed at in the following ſcheme.

Let a ſmall globe of metal be attached to the end of a line which is to be paſſed over pulleys fixed in the ſtern-poſt of a ſhip, in ſuch a manner that this globe may be drawn through the water after the ſhip at ſo low a level as not to be affected by the ſhip's wake or the motion of the rudder, and ſo that the other end of the line may be conducted into the veſſel, where it is to be joined to the end of a ſtrong ſpiral ſpring of ſufficient ſize and elaſticity to expreſs, by diſtinct expanſions and contractions, every variation of reſiſtance which may affect the immerſed globe. If a ſhip, furniſhed with ſuch an inſtrument, move at ſea with an aſcertained degree of velocity, and if the expanſion of the ſpring, at the ſame time, be marked on a ſcale annexed, this degree of expanſion will be peculiar to that degree of velocity; and if the ſcale be filled with all the uſeful graduations taken from actual experiment, the ſhip's rate of motion may be known thereby at any time, whatever the law of hydrodynamic reſiſtance may be.

Then ſuppoſe a cup, with a ſmall perforation in the bottom, containing that ſort of metallic ſand which is uſed for hour-glaſſes, to be joined to the extremity of the ſpring, the ſtream of ſand which iſſues from it will have a common motion with the ſpring, ſo that the point of the ſcale on which this ſtream falls will mark the rate of progreſs, and (ſince ſand* flows equably) the quantity which falls upon that point will mark the time, the product of which quantities is the diſtance paſſed over during that time. Therefore, if there be a receiving veſſel for the ſand, divided tranſverſely into ſeveral compartments, ſo that the partitions between them may be at the ſame diſtances aſunder as the graduations of the ſcale, the ſum of the products ariſing from the multiplication of the quantities of ſand received in all of them, by the velocities denoted by the compartments which hold them reſpectively, will be the diſtance ſailed at any time.

However, though this method furniſhes quantities from which the diſtance may be computed, conſiderable delay muſt ariſe from the meaſurement of each ſeparate portion of ſand, and the multiplication of each of theſe by its reſpective degree in the ſcale; which difficulty may be removed in the following manner:

It is evident, from the nature of the ſcale, that the orifices of the compartments of the receiver muſt be unequal; but if

* It has been proved, by experiment, that the diſcharge of ſand in this caſe will be the ſame in equal times, whether the ſuperincumbent weight be great or ſmall,—contrary to the eſtabliſhed laws of *fluids*.

they

they be made to diverge as they deſcend, ſo that they may be equal at the bottom, the unequal ſcale will be changed into a ſcale of equal parts, the ſand received into it will be ſo diſtributed thereby, when lodged at the bottom, that the diſtance of each portion of it from the end of the receiver will be as the ſhip's velocity when it fell; and, conſequently, the ſum of the products ariſing from the multiplication of the quantity in each compartment, by its diſtance from the end of the receiver, will be as the diſtance ſailed. But the ſum of theſe products is equal to the ſingle product of the diſtance of the common centre of gravity of the fallen ſand from the extremity of the receiver, by the weight of all the fallen ſand; and, if the receiver be furniſhed with a falling handle, this centre of gravity may be found at any time by finding tentatively that point of the handle by which it may be ſuſpended, ſo that the receiver containing the diſcharged ſand may not incline to either ſide: and if this experiment be made with the hook of an ounſel containing a ſcale graduated for the purpoſe, the weight or quantity of ſand diſcharged may be known; alſo, if the falling handle be graduated into equal parts, correſponding with the diviſions of the compartments at the bottom of the receiver, and marked with numbers repreſenting the rate of ſailing which theſe diviſions denote, the number on this handle at the centre of gravity, multiplied by the number on the ſcale of the ounſel, will ſhow the diſtance ſailed at any time.

In order to adapt this inſtrument to the ſeaman's uſe, it is neceſſary to conſider the irregularities of motion occaſioned by the waves, and to provide againſt their effects. Theſe are of three ſorts. Firſt *rolling*, or a motion about the longitudinal axis of the ſhip, which does not ſeem capable of diſturbing the operation of the inſtrument. Secondly, *lee-way*, which takes place when a ſhip moves in any direction except that of her keel: in this caſe, the ſtring, being drawn tranſverſely, will not have its full effect on the pulley at the bottom of the ſtern-poſt, but will act as an oblique force, and conſequently the diſtance will be repreſented too little; to remedy which, the lower pulley muſt be formed ſo as to have a motion like that of a caſtor on the foot of a table, whereby it may be turned by the line into its own direction, and then the action of the ball will produce the required effect. Thirdly, *pitching*, or a vibrating motion about the tranſverſe axis, which, if the ſtring to which the ball is faſtened be ſhort, in an high ſea, would give a ſerpentine motion to the ball, that would repreſent the diſtance greater than it ſhould be; to prevent

prevent which, the ſtring is to be of ſuch a length as to render the angle through which it vibrates very minute, whereby its tendency to deflect the ball from a rectilinear path will be diminiſhed; and if (as Mr. Boyle and others aſſert) the greateſt height of a natural wave be no more than ſix feet, it will not require a very long radius to render the angle ſubtended by a ſine of ſix feet ſo ſmall, that the divarication occaſioned by it may become inconſiderable.

This may be ſubjected to experiment in an eaſy manner: let a boat, with the inſtrument annexed, be drawn on a canal through ſuch a ſerpentine line in an horizontal plane as it would deſcribe in a vertical plane in paſſing over waves, and the effect on the ball will be the ſame in both caſes; by which means it will be poſſible to find the error occaſioned by pitching, and the length of ſtring neceſſary to reduce it.

It may occur as an objection, that the immerſed ball cannot be kept at the ſame depth at all times, and that it may receive different degrees of reſiſtance from the ſame velocity at different depths, which muſt occaſion error: but the experiments of C. Coulomb, publiſhed in the Phyſical Memoirs of the French National Inſtitute during the third quarter of the year 8, tome iii. p. 288, prove the reverſe; his words are—"L'on peut conclure de cette expérience que lorſqu'un corps ſubmergé ſe meut dans un fluide, la preſſion ou la hauteur du fluide au deſſus du corps n'augmente pas ſenſiblement ſa reſiſtance."

But the greateſt difficulty attending this ſcheme is to form the ſcale of the ſpring that is to aſcertain the ſeveral degrees of preſſure which a globe of certain dimenſions meets from the ſeveral requiſite degrees of velocity. For this purpoſe, let a man be furniſhed with a ſlender chain one yard in length, ſo faſtened between his ſhoes as to limit the length of his ſteps; let him keep his eye fixed on the vibrations of a pendulum, and, by ſtepping to the full length of his chain in time with the oſcillations, his motion will be equable, and the rate of it will be exactly known. If a man ſo prepared ſhould draw a boat along a ſtagnant canal, the rate of the boat's motion will be equable alſo, and aſcertained; and this rate may be varied to any required rate within certain limits by altering the length of the pendulum: then if the inſtrument be attached to the boat, the expanſion of the ſpring at each degree of celerity with which the man proceeds may be marked on the ſcale: for the higher degrees of velocity the man's motion muſt be increaſed by ſome mechanic power, as the axle in the wheel, whereby additional force may be converted

verted into velocity, and the degree of velocity aſcertained by the power of the machine. This experiment ſhould be made in a wet dock filled with ſea water.

It ſhould alſo be obſerved that theſe experiments will afford a ſcale which will anſwer only for balls of the ſame dimenſion as that with which the original experiment was made, and therefore the mould of the original ball ſhould be preſerved; but this is not the caſe of the ſpring, for the ſcale of any ſpring may be eaſily found when the ſcale of one is graduated in this manner:

Let the extremities of the new ſpring and of the original ſpring be joined, ſo that they may act againſt each other; then pull them aſunder until the index of the original ſpring ſhall touch ſome graduation, and then the ſcale of the new ſpring may be marked where its index then ſtands, with the degree of velocity at which the index of the original ſpring ſtands; and in this manner all the neceſſary graduations may be transferred from one ſcale to another, though the ſprings ſhould be of very different powers; becauſe, ſince the ball is to be the ſame, the force exerted by each degree of velocity remains unaltered.

However, in a very high ſea this method may be attended with ſome inconvenience: the ſtring may be frequently broken, and a certain degree of error will attend a very ſtrong pitching motion; in which caſe the following method may be uſed to aſcertain diſtance by means of theſe waves, which interrupt the operation of the inſtrument:

Let any ſmall floating ſubſtance be thrown out a-head of a ſhip at ſea, and let the number of times be noticed when it is ſeen diſtinctly on the top of a wave; alſo let the interval of time between the firſt and laſt of theſe elevations be obſerved by a ſtop-watch; which time being divided by the number of elevations will be the time in which a wave paſſes through its own breadth, (becauſe the floating body is not protruded by the action of the waves.) But a wave paſſes through its breadth in the ſame time in which a pendulum whoſe length is equal to that breadth performs one vibration *: and tables are or may be conſtructed to ſhow by the time of vibration the length of the pendulum; which tables will of courſe ſhow, by the time of undulation, the ſpace through which any wave moves in that time, and alſo the breadth of the wave †. Therefore, if a ſhip ſailing *right before* the wind were to accompany the ſame wave through the

* Quære, what effect has a current in diſturbing this law of the waves?

† Or the breadth and velocity of the waves may be found by an experiment with the log-line.

ſea,

ſea, her rate of ſailing would be known by the time of the wave's undulation: but, ſince the ſhip receives the impulſe of the wind more powerfully than the wave, it moves faſter and makes more way than a wave by the breadth of all the waves which it paſſes through; and ſince a ſhip pitches on paſſing over each wave, and ſince the number of pitches may be regiſtered, without trouble, by an oſcillating inſtrument, (like the watch which ſhows the number of ſteps which the wearer makes,) the number of waves whoſe breadth is to be added to the progreſs deduced from the tabular rate is known; whereby the diſtance made by a ſhip *ſailing before the wind* may be found, provided the time of undulation be aſcertained whenever there is reaſon to ſuſpect a change in the motion of the ſurface of the ſea. But when a ſhip ſails *upon a wind*, the diſtance deduced from theſe data requires correction. Suppoſe two ſhips placed on the ſame wave, and that one runs directly before the wind and keeps time with this wave, and that the other ſails in a courſe which forms an angle with the direction of the wind, retaining, however, her place on the ſame wave; it is evident that then the ſpace deſcribed by the former will be to the ſpace deſcribed by the latter, as the coſine of this angle to radius: alſo, if theſe ſhips, purſuing theſe courſes, paſs over the ſame waves, or the ſame number of ſimilar waves, the direct path of the former in paſſing theſe waves is to the oblique path of the latter as the coſine of the ſame angle to radius alſo: therefore, in ſailing *upon a wind*, the reſult which this method of calculation gives, is to the true diſtance as the coſine of the angle which the rhomb forms with the direction of the wind is to radius, and may be corrected accordingly: or, if this angle be aſſumed as a courſe, and the reſult obtained by this method be taken as difference of latitude, the *corrected* diſtance will be found in the diſtance column of the common nautical tables.

But both theſe methods muſt prove erroneous where the ſhip is borne away by a current; and the common method of eſtimating the effect of a current by the apparent motions of a ſunken body is fallacious; becauſe the boat may be in quieſcent water, and the current may exiſt only below; or the upper and lower ſtrata of water may be parts of the ſame current flowing in the ſame direction with different degrees of velocity: therefore any theory which points to a true meaſure of currents ſhould be announced, though it may require long experience to perfect it.

Let two ſhips *lie-to* at a conſiderable diſtance aſunder (ſuppoſe twelve miles): if a gun be fired from each from a ſitua-

tion

tion which may be four yards above the water, it may be ſeen from the other at an elevation of fifteen yards above the water; and conſequently, the interval of time between the obſervation of the flaſh and report of a gun from either veſſel may be obſerved at the other at this diſtance, which time, according to the laws of ſound, will be about 55½ ſeconds: and, if there be no current, each will obſerve the ſame interval of time between the flaſh and the report of the ſhot from the other; but if they float on a current which runs at the rate of ſeven knots an hour, for inſtance, one veſſel advances 570 feet towards the point whence the ſound iſſued during the paſſage of the ſound, and will conſequently hear it half a ſecond ſooner than if ſhe remained at reſt; whilſt the other, by receding from the former veſſel, lengthens the ſpace through which the ſound is to paſs by 570 feet, and therefore will hear it half a ſecond later: therefore there will be an entire ſecond of time difference between the obſervations of the two ſhips; and in ſuch caſe it may be inferred that the ſhip which obſerved the ſhorter interval floats on a current which runs *towards* the other ſhip, and *vice verſa:* ſo that two ſhips, making this experiment in ſeveral points, will detect the exiſtence of a current, and the true direction of it; and, if it be not too much to expect from the accuracy of future obſervers, that the ſubdiviſions of a ſecond ſhould be diſtinguiſhed, the *rate* of the current may alſo be found from the diſtance between the ſhips and the time of the ſound's paſſage from one to the other, provided this portion of time can be meaſured with ſufficient accuracy.

[To be continued.]

LIV. *On the Nature of Heat. By a Correſpondent.*

IT has long been a queſtion whether the ſenſation we experience, called *heat*, ariſes from a ſubſtance of a diſtinct nature *per ſe*, or whether it is generated from an intenſe vibratory motion of the inſenſible particles of bodies. In reaſoning on this ſubject, our arguments can only be founded on the effects that are produced by this unknown cauſe; for it ſeems more than probable that the degree of accuracy of experiment neceſſary to render our concluſions certain with regard to its intrinſic nature, is incompatible with the preſent means of our knowledge. The following obſervations are intended therefore, by directing the attention to a conſideration of ſome of the chief phænomena of heat, to ſhow that we can

ſeek

ſeek for an explanation of them in no way ſo ſatisfactorily as by ſuppoſing them to originate from the univerſal preſence of a very ſubtile fluid called by the French writers *calorique*, which penetrates, without exception, all bodies, and is equally diffuſed through all ſpace.

That heat is only a property of common matter, a ſpecific motion of the particles of bodies, is an hypotheſis perfectly inadequate to the ſolution of the phænomena alluded to. Lord Bacon is the firſt modern philoſopher who attempted to elucidate at length any theory on this ſubject; and we find him, in a treatiſe written expreſsly for this purpoſe, and entitled *De Forma Calidi*, deducing, from an enumeration of the ſeveral phænomena and effects of heat, its general properties, and hence defining it to be *an expanſive undulatory motion in the minute particles of the body, by which they tend with ſome rapidity towards the circumference, and at the ſame time incline a little upwards.*

This ſyſtem, which evidently conſiders heat not as an original inherent property of any particular ſort of body, but as mechanically producible in it, was adopted by moſt of the mechanical philoſophers of that age, by Boyle, Newton, and Deſcartes; and has been ſuppoſed to have received no inconſiderable ſhare of weight from the later experiments of Count Rumford and Profeſſor Pictet. Before, however, we acknowledge the truth of this hypotheſis, it is neceſſary we ſhould firſt aſcertain if it is capable, in its application, of accounting for the various phænomena of nature; for, unleſs theſe are found to admit by it of a ſimple and obvious interpretation, we are juſtified in concluding that it is unequal to the province aſſigned it. That the theory under conſideration will not ſtand the teſt that is here ſubmitted as the criterion of its truth, cannot be well doubted after an attention to the following facts:—If a thermometer be placed under the receiver of an air pump, and the air be ſuddenly exhauſted from it, the thermometer will ſink ſeveral degrees, and very quickly after will riſe again to its uſual height. Now if heat conſiſts in vibrations, according to the tenets of this hypotheſis, it may be aſked, How comes it to paſs that the ſmall quantity of matter that remains within the receiver is firſt inſufficient, and afterwards ſufficient, to maintain the temperature originally indicated by the thermometer? This is a fact which no reaſoning can ſatisfactorily account for ſimply on mechanical principles, and can only be reconciled on the ſuppoſition that there is ſuch a fluid as caloric poſſeſſed of a nature peculiar to itſelf.

The univerſal power of expanſion that heat poſſeſſes (for we

we are entitled to presume that, where a contrary effect is witnessed, it proceeds from the expulsion of some fluid or matter foreign to the substance on which it acts,) is another fact wholly inconsistent with the theory in review. Thus, when we perceive that heat, applied to any substance, invariably occasions its dilatation, we are justified in concluding that it is enabled to produce this effect only by virtue of its own proper extension; and if we allow that it is capable of extension, we cannot refuse our assent to its being a bodily substance. If we suppose this effect to be produced in any other way, as by the impression of any mechanical motion on the parts of the substance, it is difficult to conceive how this could produce a lasting expansion of the particles; it would excite, indeed, an undulatory or vibratory effect, occasioning a change in the situation of the particles for the time, but there would still be a tendency in them to return again to their former situation, and an oscillatory effect only would be produced, perfectly distinct from extension. It is, however, impossible to reconcile this fact with the supposition that heat is a quality, or an adventitious and accessory property resulting from the intestine motion of the particles of bodies, and not a substance of itself. "Whenever," says Mr. Locke, "we perceive a number of qualities always existing together, we are warranted in the conclusion that there is some substance which produces those qualities." If heat depended on motion, it is natural to expect that it would display a greater facility in its passage through elastic bodies than those that were more dense; yet we find that no rule of this sort obtains, but that it passes through the former equally slow with the latter. Again, if heat be generated by motion, it is reasonable to expect that its laws of propagation would be always analogous to the latter; yet it is hardly necessary to observe how few things we are more ignorant of than the rule which takes place in the progression and communication of heat in bodies of unequal temperatures.

Those who have adopted the theory of vibrations have assumed a position which there is no satisfactory evidence to justify their doing; for it does not appear in any one instance that we are able to demonstrate the existence of vibrations in heated bodies, while certain sounds will cause the most solid substances to vibrate perceptibly without any heat being apparently produced. The ingenious experiments of Count Rumford, it will not be disputed, have at least proved that fluids are very imperfect conductors of heat. That they are, as assumed by the Count, perfect non-conductors, is an inference that cannot legitimately be drawn even from his own

reaſonings; and, from the later experiments of Dr. Thompſon, and Mr. Murray of Edinburgh, is perfectly inadmiſſible. If, however, the experiments of Count Rumford to prove that heat is conveyed with much greater difficulty downwards than upwards, be allowed to be ſufficiently concluſive, how is this phænomenon to be reconciled by the theory of thoſe who make all heat to be generated by motion? The effect is not at all proportionate to the cauſe; for no good reaſon can be ſhown, according to this hypotheſis, why heat ſhould with more difficulty be made to deſcend in bodies than to aſcend in them; for the ſame motion will be communicated to the ſurrounding medium in all directions. Is not this fact irreconcileable with the doctrine alluded to? If, however, we aſſume the theory that heat is a peculiar ethereal fluid, all the difficulties that have before been enumerated will admit of an eaſy ſolution, and the phænomena of heat will be reconciled in a ſimple and beautiful manner: indeed, on any other principle it would be extremely difficult to account for moſt of the operations in chemiſtry, and it would introduce a degree of uncertainty into the theory of that ſcience that would go very nearly to deſtroy it altogether.

Thus, if we do not conſider caloric as a ſubſtance, we are no longer at liberty to conceive that it is capable of attraction. Oxygen gas, we are told, is oxygen combined with heat and light; and thus we find that, when oxygen enters into combination with any body, the two latter are always evolved. If oxygen gas and phoſphorus be preſented to each other, the oxygen combines with the phoſphorus, and the caloric flies off. This is readily accounted for on the ſuppoſition that caloric is a fluid *ſui generis*, and therefore capable of attraction; and conſequently we find this opinion very generally adopted by chemiſts, as alone able to afford an adequate ſolution of the phænomena in queſtion. The names of Boerhaave, Lavoiſier, Black, Crawford, and Fourcroy, are among the moſt celebrated partiſans of this doctrine, which, for its beautiful ſimplicity, its perſpicuity, and the eaſe with which it is applicable to all the phænomena of heat, is much ſuperior to that which ſtill divides in opinion many of the philoſophers of Europe. The ſtrongeſt, and indeed the only argument of weight that has been adduced againſt this theory, is that of Count Rumford, ariſing from ſome experiments made by him concerning the ſource of the heat excited by friction. From the accuracy with which theſe experiments were conducted, and the nature of the reſult, it certainly is not very eaſy to draw any inference that is perfectly ſatisfactory. It is evident that a change of capacity in the body, as

has

has been ſuppoſed by ſome, could not have effected the phænomena deſcribed: for no ſuch change could be diſcovered to have taken place; and if it had, it ought to have been ſufficiently great to have accounted for all the heat that was produced. In reflecting on this experiment, we cannot but admit, that with the preſent means of our knowledge we are incapable of explaining the effect that is produced; yet it would not be very philoſophical to conclude, that any ſyſtem was abſolutely falſe, which, though it readily ſolved the chief phænomena required, might, in ſome inſulated inſtance, be found apparently incompetent to the taſk*. Should we not, if our ſyſtem be previouſly founded on a broad and ſolid induction, and eſtabliſhed on the firm baſis of experiment, be led to attribute rather any deficiency of explanation in any particular inſtance, to a want of knowledge of ſome minute links in the chain, than as involving any inſuperable obſtacle to the doctrine itſelf? And ſhould it not lead us, inſtead of abandoning, to review our theory with more circumſpection and a greater caution?

No one will be inclined, perhaps, to diſpute the Newtonian theory of gravitation, yet there are phænomena which it is difficult to conceive how it accounts for. Thus it would ſeem a contradiction to the eſtabliſhed principles of our great philoſopher, to ſuppoſe that a body divided into parts, however minute, can poſſibly aſcend in a fluid ſpecifically lighter than itſelf. A little acquaintance with the operations of chemiſtry will ſhow that this is a fact that occurs in the ſolution of various ſolids, and that without any motion being communicated to the veſſel containing them; the ſolid becoming diffuſed throughout the ſubſtance of the diſſolving fluid, and appearing to overcome the natural tendency of bodies towards the centre of the earth, and to have ſome new power of aſcent impreſſed on its particles.

To conclude, then, it appears that we are entitled to conſider caloric, in the preſent ſtate of our knowledge, as a ſubſtance *ſui generis*, not becauſe we can ſenſibly demonſtrate the truth of our opinion, but becauſe it is the leaſt exceptionable theory of the two, and becauſe the phænomenon of heat admits from it an interpretation more ſimple and obvious, and more agreeable to the analogy of nature. P.

* If our correſpondent had attended to this fact, that in the experiments alluded to, the Count, with all the care and precaution he employed, had not ſucceeded in inſulating from the contact of caloric the bodies ſubjected to friction, he would not have found them ſtand at all in the way of his argument. Was not the apparatus wholly immerſed in a bath of caloric—the atmoſphere?—EDIT.

LV. *On the different Proportions of Carbon which conſtitute the various Qualities of Crude Iron and Steel.* By DAVID MUSHET, *Eſq. of the Calder Iron Works* *.

IT is of conſiderable importance to the manufacturer to aſcertain the abſolute portion of charcoal which in his proceſs becomes united with the metal to form caſt iron. Having once admitted the fact, that it is in the ratio of the carbon preſented to the metallic particles that he obtains a determinate quality of crude iron, experiment will enable him to deduce, that in manufacturing the richeſt natures of iron his produce from the ore will be more, by the extra quantity of carbon neceſſary to conſtitute this quality, than when the inferior numbers of iron are produced. In all caſes, therefore, in making caſt iron, conſiderable quantities of the coal become united with the iron, forming, by weight, a portion comparatively great.

From the uſual proceſſes hitherto employed in manufacturing bliſtered ſteel, the poſitive quantity of carbon which became united with the iron never became an object worthy of the attention of the manufacturer. It was ſufficient to him that his bars poſſeſſed bliſters ſufficiently large and prominent to aſſure him that his ſteel was ſufficiently converted. The unerring teſt of practice through a long ſeries of operations confirmed the correctneſs of this deduction; and it muſt have appeared a matter of little importance to the formation of ſteel, that its direct operation of principle ſhould be developed, or the laws which regulate its affinity in cementation.

No additional fact was neceſſary to the production of caſt ſteel. The ſimple fuſion of bar ſteel, regulated by ſuch circumſtances as practice, and the various uſes to which this ſteel is applied, was all that was neceſſary to be known; the affinity of iron for carbon, and the various proportions in which it exiſts with the metal, forming the different qualities of ſteel, here met with no elucidation.

On the contrary, we ſtill find that the union of iron and charcoal to form ſteel, is a matter of doubtful opinion among manufacturers, and the weight gained by its cementation generally denied.

It is only lately that a proceſs has been brought into uſe for making caſt ſteel, which has for its baſis or principle the direct proportions of carbon neceſſary to form ſteel, illuſtrative at the ſame time of that beautiful phænomenon of affi-

* Communicated by the Author.

nity

nity betwixt iron and carbon which conſtitutes the endleſs varieties of this metal.

To inveſtigate this ſubject with accuracy, it appeared to me that a ſeries of experiments, made by the fuſion of bar iron with well prepared charcoal of wood in veſſels completely air-tight, would be productive of the greateſt nicety of reſult. With this view, the proportions of iron and charcoal recorded in the following experiments were each of them introduced into crucibles of Stourbridge clay while the clay was yet moiſt. The top was accurately cloſed upon the contents, and the crucibles ſet aſide to dry for eight or ten days. Previous to the introduction into the melting furnace they were baked in a common annealing fire to bring them to an ordinary red heat: from this furnace, while hot, they were ſucceſſively placed in the aſſay-furnace for reduction.

The following are the details of theſe experiments:

		Grains.
Exp. I. Pieces of Swediſh iron - -		1173
Charcoal $\frac{1}{2}$, or - grs.	$581\frac{1}{2}$	
The mixture was expoſed to a moderate degree of heat for 70 minutes, when the crucible was withdrawn. When cold it was carefully examined, and found free from cracks.		
Charcoal untaken up, of a deep black colour,	$310\frac{1}{2}$	
Charcoal diſappeared in the fuſion -	271	
equal to 48 per cent. of the original quantity.		
The button obtained was ſupercarbonated crude iron, and weighed - - - -		1233
Gained in weight, by the combination of carbon,		60

equal to $\frac{1}{17}$th part fully of the firſt weight of iron.

The fracture of this button was exactly that of No. I. pig iron, towards the upper ſurface largely granulated; and below, a ſubſtratum of ſmall grained metal reſembling No. III. pig iron.

		Grains.
Total weight of the mixtures -		$1754\frac{1}{2}$
Charcoal remaining	$310\frac{1}{2}$ grains.	
Iron obtained -	1233 grains.	1543
Total loſs in fuſion		211

The quantity of charcoal which diſappeared in this operation was equal to $\frac{1}{4\frac{3}{17}}$th part the weight of the iron.

		Grains.
Exp. II. Swediſh bar iron - -		1093
Charcoal $\frac{1}{4}$, or - grs.	$273\frac{1}{4}$	

	Grs.	Grs.
Brought over	273¼	1093
The fusion of this mixture was effected in fifty minutes. When cold, the crucible was found entire, and containing of charcoal not taken up	90¼	
Lost in the fusion	183	
equal to 67 per cent. of the original weight of charcoal. A fine button of supercarbonated crude iron was found, which weighed - -		1143
Gained in weight by combination of charcoal		50

equal nearly to $\frac{1}{2\frac{1}{3}}$d part the weight of the iron employed. The fracture of this button was inferior in point of lustre and prominency of crystal. It was, however, similar to the usual run of the No. I. iron of the manufacturer.

The quantity of charcoal which disappeared in this experiment was equal to $\frac{1}{7}$th part the weight of the iron.

		Grains.
The aggregate of the mixture weighed		1366¼
Charcoal remaining	90¼ grains.	
Iron obtained -	1143 grains.	1233¼
Lost in weight upon the whole		133

Exp. III. Swedish iron in pieces - - 1143
Charcoal $\frac{1}{7}$th, or - grs. 190

This mixture was fused in thirty minutes, and was allowed to cool to a low red heat before it was removed from the furnace. When cold, the crucible was found without cracks, and the charcoal not taken up weighed - -	55
Charcoal disappeared, equal to 71 per cent.	135
The metallic button now obtained was superbly carbonated so as to resemble a mass of carburet of iron. It weighed - - - -	1193
Original weight of the iron - -	1143
Gained by combination of charcoal	50

equal nearly to $\frac{1}{2\frac{1}{3}}$d part the weight of the iron. All the surface presented by this button was uniformly covered with a rich and lustrous coating of carburet, indicating a superior quality of crude iron. The quantity of charcoal which disappeared on this fusion was equal to $\frac{1}{8\frac{1}{2}}$ part the weight of the iron employed.

The

	Grains.
The aggregate of mixture here weighed -	1333
Charcoal not taken up 55 grains.	
Iron obtained - 1193 grains.	1248
Total loſs in the melting	85

It is worthy of remark, that the loſs of charcoal in theſe experiments, together with the general loſs in fuſion, diminiſh in a ſeries proportionate to the quantity of charcoal introduced, bearing no perceptible relation to the quantity of iron. The weight gained by the metallic buttons reſpectively, indicate nearly the ſame degree of ſaturation of the carbonic principle, whatever difference of appearance exiſted as to external appearance in No. III. It therefore appeared a matter of ſome curioſity, worthy of inveſtigation, what became of the extra loſs of carbon in No. I. and II. beyond that ſuſtained in No. I. when the richeſt iron was produced.

Charcoal diſappeared	No I.	II.	III.	
Equal to - - -	$\frac{1}{4\frac{3}{10}}$	$\frac{1}{6}$	$\frac{1}{8\frac{1}{2}}$	
Gained by iron -	$\frac{1}{19}$	$\frac{1}{22}$	$\frac{1}{23}$	Average $\frac{1}{21\frac{1}{3}}$d part.
General loſs in fuſion	211	133	85	Grains.

Exp. IV. Swediſh iron - - - 1060
Charcoal $\frac{1}{10}$th, or 106 grains.
From this mixture a very perfect fuſion was obtained, in which all the charcoal diſappeared except about $\frac{1}{2}$ grain, compoſed of ſmall granules poſſeſſing a rich deep black colour. The metallic button weighed 1093

Gained by the combination of charcoal 33
equal to $\frac{1}{33}$d part the weight of the iron employed. In addition to the weight of the button, the ſides and top of the crucible contained a conſiderable number of minute ſpheres of iron poſſeſſing priſmatic colours. The ſurface of the preſent product was cryſtallized in radii. The fracture was that of highly blown crude iron of a pale ſilvery white colour, marked with an imperfect cryſtallization. The quantity of charcoal which diſappeared in this operation was equal to $\frac{1}{10}$th part the quantity of iron at firſt introduced.

	Grains.
The aggregate weight was - -	1166
Charcoal remaining $\frac{1}{2}$ grain, and iron obtained 1060	$1060\frac{1}{2}$
Total loſs in this fuſion	$105\frac{1}{2}$

Exp. V. Swediſh iron - - - 1000
Charcoal $\frac{1}{15}$th, or - - 66
This fuſion was found complete after half an hour's expoſure.

The charcoal had totally disappeared. The metallic button was regularly crystallized, and weighed 1000 grains, being exactly the same weight introduced. Besides the button, some thousands of metallic globules entirely covered the sides and concave top of the crucible. It was found impracticable to collect them all. By estimation they appeared to be from 15 to 20 grains. The fracture of this button was whitish blue, clear, and possessed a lustre similar to the fracture of zinc. An approximation to grain was visible, and, from minute comparison, a very early stage of steel was indicated.

	Grains.
The aggregate of the mixture weighed -	1066
Button of iron 1000, globules taken at 20, -	1020
Total loss in the fusion	46

Exp. VI. Swedish iron - - - 1035
Charcoal $\frac{1}{20}$th, or - $51\frac{1}{2}$ grains.
The mixture yielded, by fusion, a metallic button partially crystallized, weighing - - 1032

Lost in fusion, equal to $\frac{1}{31\frac{1}{2}}$th part, 3

The charcoal had entirely disappeared. The fracture of this button displayed a very regular grain, similar to that of steel. In subjecting it to forging and other tests, it proved to be steel of a soft quality.

	Grains.
Original weight of mixture -	$1086\frac{1}{2}$
Iron obtained - - -	1032
Total loss in fusion	$54\frac{1}{2}$

Exp. VII. Swedish iron - - - 1055
Charcoal $\frac{1}{30}$th part, or 35 grains.
A very perfect fusion was obtained from this exposure. The metallic button was found beneath a covering of glass possessed of a white streaky surface, the mass of which was of the transparency and colour of a smoky topaz: it was found to weigh - 1052

Lost in fusion, equal to $\frac{1}{351}$ part the original iron 3

In this experiment, also, the charcoal had completely disappeared. The present product was minutely examined, but gave no indications of steel. After being deeply cut with a chisel, it was broken with very great difficulty across the anvil. It was then forged, and plunged hot into water; but did not harden. It generally resembled those qualities of iron obtained by fusion with earths and glasses. This experiment was repeated four times, and always attended with a similar result;

result; so that it seemed deducible from it, that $\frac{1}{3}$th part of carbon in addition to any quantity of iron was insufficient to form steel; and, referring to the result of No. VI., it appeared that even $\frac{1}{20}$th part formed a steel much too soft for the general purposes of manufacture. This conclusion, however, being at variance with facts I had already established upon the formation of cast steel in common crucibles, was here inadmissible. It was therefore necessary to seek for an explanation of the phænomenon of the charcoal disappearing in close vessels, formerly alluded to, before any certain knowledge of the exact quantities of charcoal could be ascertained, which were necessary to form either cast iron or steel in vessels made impervious to the air *. I uniformly remarked in the present experiments, that when the quantity of charcoal introduced was from $\frac{1}{30}$th to $\frac{1}{200}$th the weight of the iron, a portion of glass was constantly formed upon the surface of the metallic button. The quantity generally increased as the proportion of charcoal decreased; so that, in some experiments, 300, 350, and 400 grains of amber-colour glass was obtained. The upper surface of this glass was frequently of a pure pearly white colour. In one experiment, where $\frac{1}{70}$th of charcoal was used, I obtained a large quantity of glass cellular throughout. Each cell was surmounted upon the top with concentric circles of pearly lines, forming a curious and pleasing effect.

Having fully satisfied myself that operations performed in close vessels thus prepared were subject to uncertainty, arising as well from the formation of glass as from some unknown affinity exerted upon the charcoal; and having performed several experiments with well filled open crucibles, with charcoal alone, wherein I found little comparative loss, I performed a very accurate series of experiments, which shall be forwarded for the next number of the Magazine.

LVI. *On the Fate and Character of the Monk* Roger Bacon. *By Governor* Pownall. *Read before the Literary and Philosophical Society, Bath* †.

THERE has been much bustle about learning in the world: every age has had its learning; the produce and growth of a desultory activity of the imagination, according to the fashion of thinking prevalent at the time: whilst the

* This investigation will form a separate paper for July.

† Communicated by the Author.

ſlow ſilent progreſs of the reaſoning power is hardly ever noticed in its operations, and ſeldom received in its effects when they come forward, becauſe not underſtood at the time.

It would be a curious detail were we to enumerate the various fancies and follies which have been called learning, which have been ſtudied and applauded as ſuch in every age and every country; but it is at the ſame time a humiliating truth, that knowledge, which it ſhould ſeem is congenial to the right operation of the human intellect in the inveſtigation of truth, has come forward to men but ſlowly and ſparingly, and at epochas of very diſtant periods, and at the intervals of very diſtant centuries: and what is more humiliating in the hiſtory of man is, that, when at any time it appears, it comes into the world as an *enfant trouvé*, and is received as ſuch, rather than nurtured by thoſe whoſe relation to it ſhould be in practice, as it is in nature, congenial to it. True knowledge is therefore generally either deſpiſed by the conceit of the learning in faſhion, or more generally ſuſpected as dangerous; which, indeed, it always is to the falſe and artificial authority which aſſumes and takes the lead in the world.

It is with pain that I have to notice ſeveral eminent examples of this melancholy truth in the courſe of my ſtating the character and fate of the monk Roger Bacon, who is the ſubject of this paper.

When Socrates came forward to the world, the powers of the human mind, abſorbed in the falſe learning of Greece, had been employed to propagate the ſophiſtry and ſupport the ſophiſts of the time, whoſe ſyſtem and buſineſs were to keep mankind from true knowledge by an avowed principle of ſuch ignorance as was incapable of attaining truth; the purport of which ſyſtem was to hold men in a ſervile obedience to authority, as thoſe who had the command of that authority ſhould impoſe it. Socrates in his theology taught only thoſe truths which had been known to the antient philoſophers of Greece, and which they had brought from the eaſt. What they affected to conceal he taught openly, ſo far as went to eſtabliſh the knowledge of *a moral ſyſtem* connecting the divine and human nature. *Here he was original:* this was a new truth brought forward to the world. In this, as it is truly ſaid of him, he brought down truth from heaven, and eſtabliſhed it on a moral foundation on earth.

As to his manner of teaching, he with great addreſs, in accommodation to the learning of the time, profeſſed to commence in ignorance. But whilſt the ſophiſts led, in ſynthetic reaſoning, from ignorance to a total incapacity in man for truth; he, commencing his theorem from ſtated

ignorance, proceeded by *analytic induction* to the eliciting of truth: as the algebraiſts have ſince done, from a theorem ſtated on an unknown quantity, bringing out the true one. He thus not only brought truth to light by reaſon, but by the very mode of his reaſoning taught the uſe, the extent, and the right of human reaſon. This was ſufficient to decide the fate which both the teacher and his doctrines experienced. His doctrines were deſpiſed, and attempts were made by the wits of the time to ridicule his knowledge: it was repreſented as evolved from the clouds*. But, as truth will ſtand the teſt of ridicule, nothing remained but *to decide by authority* that the truths which he taught were inadmiſſible; and they were therefore *interdicted by power*: and Socrates himſelf was proſecuted and condemned to death as a falſe teacher, as a corrupter of men, an heretic, unbeliever, and deſpiſer of the gods! What is ſtill more humiliating to the character of man, the doctrines which he taught, and the truths which he publiſhed, were perverted where received, and even by his diſciples. The homely ſimplicity of his doctrines was ſophiſticated and corrupted, in a line of learning, to uſes perverſive of their nature.

I will not here glance, although it is in my mind's eye, at the ſimilar fate of a Divine teacher who came forward in the world to enlighten man, and to inſtruct him in his moſt eſſential duty and intereſt.

Purſuing therefore the fate of Socrates, which is not irrelative to the matter of this paper, I ſhall proceed to ſtate that Plato, his favourite diſciple, who was a really great mathematician, and particularly converſant in the *analytic proceſs* of that ſcience, and who followed the ſteps of induction marked out by Socrates, mixed ſo many myſtic conceits and theories, that he ſublimed and ſubtilized truth to a mere learned viſion, ſuch as the latter Platoniſts afterward taught.

Ariſtotle, on the other hand, a diſtinguiſhed diſciple of Plato, converted the knowledge of phyſics, both material and intellectual, into *an artificial ſyſtem* of axioms, data, and maxims, and formed thereon *a logical frame* of ſyntheſis; which gave occaſion to his immediate followers and to the Peripatetic ſchool to found their learning on the authority of teaching, inſtead of being founded on *truth* led by experience to knowledge, to the diſgrace of human intellect, and the perverſion of true philoſophy.

It is lamentable to reflect how many theories and myſteries, divine as well as natural, ſprang from this foul ground;

* Vide the Nubes Ariſtophanis.

which,

which, like meteors, blazed in the regions of learning for a time, but, like ſuch meteors, not deriving from true and permanent light, from proper ſources, were ſoon extinguiſhed.

Truth, however, was purſued with great energy, *occulto velut arbor in ævo*, in the ſtudies and labours of thoſe few votaries of it who were called mathematicians; and many detached unconnected diſcoveries were made, which hardly ventured to ſhow themſelves, or, where they did, were imputed to magic.

At length, after many ages of darkneſs, Roger Bacon, that great luminary, was born in 1214. He, like another Socrates, in the ſpirit of truth, in the analytic line of true knowledge, endeavoured to ſtate the *Impedimenta ſcientiæ*, the *Cauſas ignorantiæ*, and to remove all the obſtructions which ſhut up the road to truth: he then, explaining by fact and example the *Utilitas ſcientiarum*, advanced, by an experimental inveſtigation of nature, and by an analyſis of truth, to his *Reſtauratio humani intellectûs*, and his *Inſtauratio ſcientiæ*.

Reaſoning, however, above the ſcale of the learning of the age in which he lived, his doctrines were neither underſtood nor received; and, where they could not be repreſſed by the learning of the time, *they were interdicted by the ruling authority* of it. Although the truths which he diſcovered, and endeavoured to diſcloſe to the world, were liſtened to by ſeveral men of knowledge, and ſome of them in power *, and though they were received even by the *ſupreme* † authority, held infallible; yet, by *a ſin againſt the holy ſpirit of truth*, they were unacknowledged, and ſuffered to languiſh in obſcurity. They, with the teacher, were delivered over to perſecution; truth was extinguiſhed, and the diſcoverer and teacher impriſoned for the crimes of magic and hereſy.

Here again, on the ſtage of this world, was truth devoted to ſilence and darkneſs, and the diſcoverer and teacher of it thrown back into obſcurity, and his doctrines loſt in oblivion, except what ignorance rakes up to memory in diſgraceful tales of tradition. The ſtream, therefore, of this original ſpring, whence the knowledge of latter times derives its ſource, ran, like the Rhone, concealed and unknown for three or four centuries; whilſt in the mean time various empirics in philoſophy, various baſeleſs theories, (amidſt which the ingenious fables of Deſcartes more eſpecially,) took the lead and aſcendancy in the learning of the world. This ſtream, however, like the Rhone, emerged again to light on the face

* Biſhop Groſthead. † By the Pope.

of the earth, when its courſe became directed with happier ſucceſs, and under more fortunate auſpices, by the energy and ſpirit of *another Bacon*, the great lord St. Albans, who, although perhaps an original alſo, was a ſecond and more fortunate one; yet even not generally known and acknowledged as ſuch till near half a century was elapſed from the time in which he wrote.

Although Sir Iſaac Newton had made the chief part of his diſcoveries on light and colours, and had written the firſt parts of his Treatiſe on Optics, and, "at the deſire of ſeveral gentlemen of the Royal Society, ſent them to the ſecretary of that ſociety, where they were read in 1675; and although the other parts were added about 12 years after; yet to avoid, as he ſays, being engaged in diſputes about theſe matters, he delayed the printing and publiſhing of them to 1704; and would ſtill have delayed, had not the importunity of ſome friends overcome the repreſſion which he experienced from the jealouſy of ſome authoritative leaders." Such is the invariable ſpirit and temper of man even in an enlightened age, and ſuch is the unavoidable fate of thoſe who would enlighten and aſſiſt the knowledge of the world. Such, at leaſt, were the feelings of this great and humble ſpirit; ſuch was his conviction, that the age in which he firſt made his diſcoveries was neither willing to receive nor capable of poſſeſſing them, to the purpoſe of truth, that the world might have been deprived of them for ever, as in fact it is at this moment of ſome diſcoveries which he had in view and in purſuit, but which he ceaſed to follow up, and has ſuggeſted only in queries. What a loſs to mankind that theſe ſuggeſtions, except in ſome particular inſtances, have not been purſued! For, whatever and howſoever great the diſcoveries which have been brought forward by the new philoſophic chemiſts may be, yet theſe important ſuggeſtions would, as they have done in ſome inſtances where they have been taken up, have led to great and intereſting philoſophic facts hardly at preſent conceivable.

It was from the ſame opinion of the temper and want of qualification in that age, perhaps alſo from an oppreſſion of ſpirit which he felt, that on another occaſion, when conſulted by Dr. Bentley, at the time of his being appointed to preach the Boyle's lectures, Sir Iſaac ſays in his letter, "There is yet another argument in proof of the Deity, which I take to be a very ſtrong one; but until the principles on which it is grounded are better received, I think it more adviſeable to let it ſleep." Such has ever been the fate of knowledge, both to the diſcoverers and to truth itſelf, on its firſt

appearances

appearances amongſt thoſe who hold the lead of learning. Shall we then wonder at the fate which this true and real philoſopher, the monk Roger Bacon, experienced in an ignorant, ſuperſtitious, and ſervilely debaſed age?

To point out the real merit and character of this wonderful man, which is the purport of this paper, I ſhall now proceed to a review and general analyſis of his doctrines as ſtated in his *Opus Majus*.

I truſt it will be found to be an amazing extenſive plan, founded in experience of nature and on the analyſis of truth; conducted by the moſt penetrating inventive faculties of induction, and the moſt exact preciſion of combination; that it is, as I ſaid before, a *Reſtauratio humani intellectûs*, and an *Inſtauratio ſcientiæ*.

I. It firſt ſtates the cauſes of error, and then clears the ground of them; diſcards all pretenſions to knowledge which obtrude themſelves under the form of learning.

Sufficit nobis in inquiſitione proprii intellectûs ut, quantum poſſimus, cauſas et occaſiones erroris extraneas longius à debilitate ſenſûs noſtri religamus. Quatuor verò ſunt maxima comprehendendæ veritatis offendicula, quæ omnem quemcunque ſapientem impediunt; et vix aliquem permittunt advorſùm titulum ſapientiæ pervenire.

1mo. Fragilis et indignæ authoritatis exemplum.

Mark here the ground on which, in thoſe days of eccleſiaſtical and civil tyranny, the crime of hereſy againſt the church, and of treaſon againſt the ſtate, might be imputed, and which was the real ground of his perſecution and impriſonment.

2do. Conſuetudinis diuturnitas.

3tio. Vulgi ſenſus imperiti.

4to. Propriæ ignorantiæ occultatio cum oſtentatione ſapientiæ.

His omnis homo involvitur, omnis ſtatus occupatur.

II. Theſe four points he examines, explains, and diſcuſſes ſpecially and at full length under their ſeveral heads. The ground thus cleared, and the foundations thus laid, he proceeds in the *ſecond part* in analyſing grammar to its elements; wherein he not only goes to the art of language, but to the powers and operations of the mind, by an inquiſition into the human underſtanding, as Mr. Locke, and Mr. Harris in his Hermes did afterward.

Then, paſſing by the commonly received mode of reaſoning, as it moves in the trammels of logic, which this great original ſtates to be not only perverſive, but at beſt a mere left-handed hold by which we take truths already diſcovered;

by

by which we lock them up in an artificial ſyſtem, and which is of no uſe or aid in the purſuit and the diſcovery and invention of truth; he then treats of the virtue, power, and uſe of mathematics, properly ſo called, both analytic in the inveſtigation of truth, and ſynthetic in conducting it to practical ſcience.

4. By this conductor he goes firſt through the inveſtigation, and next through the didactive explanation and proof of the powers and operations of nature, by the principles of the laws of motion, attraction, gravity. Guided by theſe principles he proceeds to the laws of viſion, perſpective, and optics in general; and to the laws by which light is acted upon. And here he goes to the mechanical and inſtrumental, ſo as to explain the practical part, bringing forward many improved uſes and applications leading to new diſcoveries.

5. He proceeds hence to aſtronomy, in an inveſtigation of the phænomena of the heavenly bodies. In this part ſome remains of aſtrological prejudices not yet eradicated obſtruct his inſtitute.

6. The heads above ſtated take up five parts out of ſix into which his *Opus Majus* is divided. He then, in the *ſixth part*, comes to the main ground and true baſis on which the *Inſtauratio ſcientiæ* is founded and built up, which is *experimental induction*. This he explains in detail, and illuſtrates by example; recurring again and again to the phænomena of nature terreſtrial and celeſtial:—* Duo ſunt modi cognoſcendi, ſcilicet, per argumentum et experimentum. Argumentum concludit, et facit concludere quæſtionem; ſed non certificat, neque removet dubitationem, ut quieſcat animus in intuitu veritatis, niſi eam inveniat viâ experientiæ.

Et quia hæc ſcientia experimentalis *à vulgo ſtudientium eſt penitùs ignorata*, ideo non poſſum perſuadere de ejus utilitate, niſi ſimul ejus virtus et proprietas oſtenduntur.

Whoever, diveſted of preoccupation and prejudice as they ſpring from the *Conſuetudinis* † *diuturnitas*, and the faſcination of received faſhion in thinking, reads this great work, with comparative reference to the *Inſtauratio ſcientiarum* of the ſecond Bacon, the lord St. Albans, will find both works ſimilarly grounded, and ſimilarly conducted, as to the method and form; both alike built up on an inſtitute of induction by *experimental ſcience*; both in the execution purſuing the ſame line to the ſame point; and muſt conclude either, according to the old proverb, "*that great wits jump*;" or that the latter Bacon, although perhaps equally an original as the

* Cap. 1 and 2. partis 6tæ, de Scientiâ experimentali.

† Explained in the ſecond head of part i.

firſt,

firſt, had read and received a reflected ray of illumination from the works of the firſt Bacon, our philoſopher.

Theſe works of the monk had been buried, amidſt the manuſcripts of ſuch learning as is never read, for three or four hundred years before the time in which the lord St. Albans wrote; and continued in the ſame ſtate for near a century after, to 1733, when they were publiſhed by the learned editor Dr. Jebb; yet they might not have eſcaped the ſcrutinizing, induſtrious, inquiſitive genius and informed ſtudies of the ſecond great philoſopher. But let me here repeat, that whatever cauſe or chance firſt wrought this ſecond great genius into operation, he was equally an original as the firſt. For, whoever conſiders the proceſs of the operations of intellect, and the origin and progreſs of knowledge, will find it difficult, if not impoſſible, to mark what that ſpring, internal or external, is which firſt becomes a cauſe from a coincidence of ideas that gives courſe to it.

As it hath become an almoſt generally received opinion, that the inſtauration of true philoſophy in the preſent age derives its ſource primarily and ſolely from Bacon the lord St. Albans, I cannot but think it juſt in this paper to endeavour to reſtore to the *firſt original* that portion of merit which is due to him; and fairly to appreciate the former, without derogating from the latter: and to recommend to this ſociety to trace back the origin of their philoſophy to a more remote ſource, ariſing within the bounds of the county in which this ſociety is eſtabliſhed *; from a ſpring which, however *illaudatus*, unhonoured, and almoſt loſt in antiquity, will always be a peculiar honour to this county, as generally ſo, in every extent and degree, to the Britiſh nation at large.

Almoſt all ſocieties, originally from ſuperſtition, but ſince from the habit of cuſtom, have adopted ſome ſuppoſed patron ſaint. Inſtead of adopting a patron, let this ſociety avow as their pattern the philoſopher monk. Though no ſaint, it would be an honour to any philoſophic ſociety to adopt his *principles*, and to follow his example.

1ſt, To adopt his principles by an independent ſpirit of philoſophizing, which will not be repreſſed or kept down by the authority of any name howſoever great; will not be reſtrained within the bounds of any ſociety or ſect howſoever reſpectable; will not ſubmit to any dominant lead, but, without oppoſition or faction againſt others, will unabated follow the ſteps of truth with undeviating inveſtigation; by an ar-

* Roger Bacon was born near Ilcheſter.

dour

dour which knows no repulſe, yet with an humility of diffidence which knows no preſumption.

2dly, By a temper which repels every tincture of faſhion howſoever honoured by the times or conſecrated by antiquity, *conſuetudinis diuturnitate.*

3dly, By a judgment which, whilſt it reſpects the experience of mankind, deſpiſes the prejudices of the vulgar, great or ſmall.

4thly, By a true pride, which, conſcious of what it is capable of, knows how to commence its firſt ſteps to knowledge, by feeling and acknowledging its ignorance; and not by a falſe ſhame attempting to hide it, and with a ſtill more ridiculous vanity labouring to clothe it in the ſemblance and under the garb of learning. Of this tribe of learned gentlemen our monk ſays, "*Apparentia ſolùm tenet eos, non curant quid ſciunt*; ſed quid videantur ſcire coram multitudine inſenſatâ."

Having thus adopted his principles, let us *follow his example alſo* in the mode of philoſophizing; 1ſt, By a patient induction and cautious combination of particulars, without being haſty to form them into ſyſtems on the firſt flattering ſemblance of diſcovery: 2d, That we do not ſuborn aſſumed experiments to bear falſe witneſs to ſuppoſed facts; but to take more merit from the truth diſcovered than from the diſcovery; and to eradicate, if it be poſſible, all vanity from the heart, *et veritatem ſine dote quærere.*

Laſtly, It will not be beyond the ſcope of this paper, and, I would hope, not offenſive to this learned ſociety, to ſuggeſt, whether amongſt their other learned ſtudies and philoſophical reſearches there may not be many a commenced, though interrupted, inſtitute of experiments; many a glimpſe of diſcovery not yet brought out to open day in the works of this great original philoſopher, well worth their attention for examination at leaſt: yet

Non jurare in verba magiſtri.

I ſhall not here take notice of the diſcovery of lenſes, nor of their firſt uſes in magnifying objects, and particularly as reading-glaſſes; becauſe the refraction and reflection of light, and of the rays of the ſun, by lenſes and mirrors, were known to the antients *, though I do not meet with any mention of their uſe to aid in *reading*. But I believe the antients wrote in ſuch uncials as ſuited the powers of the eye without this aid. Another reaſon why I do not take notice of this as a diſcovery to be imputed to Bacon is,

* Vide Scholiaſt to the Nubes of Ariſtophanes.

that

that he mentions the power and operations of glaſſes on light, (if I * remember and underſtand him aright,) rather explaining theſe operations already in uſe, than as a diſcovery made by himſelf. I think, however, that it is clearly a fact, that, like Sir Iſaac Newton, and like our Herſchel, the honour of the preſent age, he made thoſe arrangements which produced the teleſcope; and that, like them, he applied his improvements to aſtronomical obſervations. He particularly mentions a previous trial which he made, as our common mechanic opticians do, of reading ſmall writing at a very great diſtance.

I do not mention the diſcovery of *gunpowder* as imputed to our monk, becauſe, although he † gives the mode of making this powder; and deſcribes its exploſive and coruſcant power; and clearly points out that he had in his mind a deſtructive uſe to which it might be applied in *comburation*, made to burn at a great diſtance: yet he withholds himſelf from explaining *this uſe*; and does not in any part mention its exploſion, as applied by the *inſtrumentality of tubes* to a projectile force that was to throw the miſſile weapons of cogles or balls. Which application of it renders it ſpecifically *gunpowder*. His words are: "In omnem diſtantiam, quam volumus, poſſumus artificialiter componere *ignem comburentem* ex ſale-petra et aliis." What theſe other articles are he mentions in another part of his works, namely, *ſulphur* and *charcoal*.

I cannot reſtrain myſelf from mentioning the idle traditional ſtory of friar Bacon's *brazen head*, which was to ſpeak and announce time, becauſe I think it founded in a real though miſtaken fact. From conſidering what one may elicit out of the nonſenſe of it, I have perſuaded myſelf that it was an experiment in horology, and the model of a clock meaſuring time by the oſcillation of a pendulum, and by various movements in *the face* of this head, (as is actually the exiſting fact in the clock at Baſle,) marking the diviſions of time, together with a mechaniſm that ſhould announce by ſound thoſe diviſions, which, in alluſion to the *face* and *head*, was called ſpeaking; ſimilar to the crowing of the cock on the clock at Straſbourg, or to the cuckoo on the wooden clocks which the Germans ſell about our ſtreets.

There remains ſtill to be mentioned, inconteſtably to his unrivalled honour, that he was the firſt diſcoverer of the error of meaſuring time by a falſe period of the year; and the firſt projector of the reform of the calendar and other

* Removed from my books, I quote from memory.

† In his tract De Secretis Operibus Artis et Naturæ.

tables

tables of time, founded on his own aſtronomical obſervations; which reform he propoſed to pope Clement V. It was the fate of this propoſal to be neglected; whilſt the imperfect Gregorian reckoning, which it hath been found neceſſary ſince to correct, was adopted. I will cloſe this paper with a tranſlation of the words of Dr. Jebb, the learned editor of the *Opus Majus*, as they ſtand in his preface:—"The propoſal made by Bacon to the pope Clement was one of the nobleſt efforts of human induſtry: conſidered in all the circumſtances under which it was framed and compoſed, it is one of the moſt ſtupendous inſtances of the force of human genius which hath been recorded; and will do honour to our learned countryman as long as the ſun and moon endure."

LVII. *Obſervations which ſeem to prove the Neceſſity of obſerving and meditating a long Time before any deciſive Opinion is formed in Philoſophy in general, and particularly in regard to the Cauſe of Earthquakes.* By COUREJOLLES*.

HOWEVER painful may be the reſearches neceſſary for diſcovering the cauſes which Nature employs in her operations, we muſt obſerve with great attention and indefatigable aſſiduity in order to collect thoſe facts which are propereſt for conducting us to the attainment of this object. It does not belong to men to invent cauſes; they exiſt in nature, and it is their buſineſs to diſcover them.

If in the exact ſciences the preciſion of a reſult eſcapes us, and if we cannot extend our conception but to that term where incommenſurability of dimenſions ſtops calculation, how can the creators of ſyſtems pretend to the privilege of penetrating to the bottom of the abyſſes where Nature has concealed her primitive cauſes? Nothing then but experience, obſervation, and compariſon, guided by geometrical method, can conduct us with wiſdom in our reſearches.

Senſible of this truth, I have thought it my duty to follow this progreſs in my obſervations on every thing that may relate to the cauſe of earthquakes; for it would be imprudent to aſſign one before a variety of facts are eſtabliſhed. This method is the more certain, as Nature often ſpontaneouſly unveils herſelf: but a long time is neceſſary to collect good obſervations: and we ought never to be precipitate in making them appear, becauſe in the ſtudy of the ſciences there is an age at which the paſſions blind us by a too ardent deſire

* From the *Journal de Phyſique*, Pluvioſe, an. 10.

of acquiring an early reputation; and, when we have obtained it in that manner, too great a degree of ſelf-love obliges us to maintain our errors, and to cover them with darkneſs, that we may preſerve them from the attacks of truth. From this weakneſs of the human mind, we may judge how dangerous we become to thoſe deſirous of being inſtructed, when, under the ſhield of a reputation, we employ every means in our power to perpetuate an erroneous opinion. Men would be much happier than they are, if thoſe whoſe documents they follow had proceeded only ſtep by ſtep in their reſearches, and had been able to free themſelves from thoſe tranſports of pride which they improperly confound with that kind of ſelf-love which conducts to good. They ought to have the courage to retract their errors; but unfortunately we have no ſchool to teach us to employ a few words ſo difficult to be pronounced—"I am in the wrong."

The habit of reflecting on, and comparing for a long time, a great number of facts reſpecting earthquakes, might embolden me to pronounce in regard to the manner in which ſubterranean matters inflame, either by the decompoſition of water and pyrites, or by other cauſes, neither the nature nor movement of which is perceived. I, however, acknowledge my inſufficiency to be ſuch, that I dare not venture to decide on the real cauſe of earthquakes. Had I liſtened only to the voice of ſelf-love, I ſhould ſoon have fabricated a ſyſtem; but I cannot do better than ſubmit my remarks to the examination of the moſt acute philoſophers, who, perhaps, will ſucceed much better than I could have done. I ſhall therefore give an extract from a very extenſive work which I compoſed on earthquakes, but which having been loſt by the burning of Cape François, I can only mention the principal obſervations, which may ſerve as data to thoſe employed in ſimilar reſearches.

1. Earthquakes are more ſtrongly felt in all parts which front the weſt, than in thoſe which have a different ſituation.

2. The earthquakes felt in diſtricts of great extent which face the north have never occaſioned ſhocks ſufficiently ſtrong to throw down houſes.

3. Diſtricts which face the ſouth have frequently experienced thoſe violent ſhocks which occaſion great damage; but this ſituation is leſs dangerous than that of diſtricts which front the weſt.

4. Diſtricts which look towards the eaſt are ſometimes ſubject to ſlight earthquakes.

5. Large iſlands, ſuch as Madagaſcar, Borneo, Ceylon, Java, Celebes, the Moluccas, the Philippines, the Japan

iſles, the Great Antilles, and all the other extenſive iſlands of the globe, experience earthquakes in the ſame manner as continents; that is to ſay, with more violence in the diſtricts which front the weſt and the ſouth, than in thoſe which look towards any other quarter.

6. Small iſlands, in conſequence of their ſmall extent, feel the effects of earthquakes almoſt in an equal manner throughout all their parts: it is obſerved, in particular, that the moſt violent ſhocks are always towards the weſt and ſouth.

7. Parts of the earth confined between two ſeas, as the iſthmuses of Porto Bello, Suez, and ſeveral others, experience the effects of earthquakes on the two oppoſite ſides.

8. In a great extent of coaſt which looks towards the north, when changes of direction occur ſo as to look towards the weſt, as that of the northern part of Africa towards Oran, or as that of the gulph of Marecaybo, earthquakes are accompanied with peculiarities analogous to their poſition; that is to ſay, thoſe parts of the coaſts which look towards the weſt are ſubject to very violent earthquakes.

9. Small iſlands lying to the weſt or ſouth of continents, or of other iſlands much larger, experience very violent ſhocks. Several iſlands which were in this ſituation have diſappeared in conſequence of violent earthquakes, by which they were ſwallowed up in the ſea.

10. Among the coaſts which face the weſt or the ſouth, there are places where earthquakes are felt with more violence than in others: it is almoſt always in theſe places that the moſt violent ſhocks are repeated.

11. Great earthquakes are almoſt always preceded and followed, ſome time before and after, by ſmall ſhocks.

12. Two earthquakes have often taken place on the ſame day at two places at a conſiderable diſtance, and ſeparated from each other by ſea or by intermediate lands which have not experienced the leaſt ſhock.

13. Great earthquakes almoſt always take place after long rains.

14. Earthquakes have often been followed by malignant fevers and epidemical diſeaſes.

15. Earthquakes are ſometimes preceded or followed by hurricanes.

16. We had a very violent hurricane at Cape François about an hour before the famous earthquake on the 3d of June 1770. This hurricane came from the mountain called La Charboniere, more than 40 leagues diſtant from the Cape. The earthquake overturned this mountain, ſo that many of the inhabitants, negroes, and cattle, were ſwallowed up.

17. The water barometer eſtabliſhed in my great meteorograph indicated on that day a depreſſion of two inches eight lines: in general, it fell only two inches during the greateſt variations.

18. The ſtorm experienced throughout the whole iſland of St. Domingo in 1788, proceeded alſo from the mountain De la Charboniere. This I aſcertained in the following manner: A few days after the ſtorm, having made a tour through the iſland on a particular miſſion, I ſaw in every place where I paſſed that the trees overturned by the hurricane were lying in the direction of radii proceeding from the ſummit of the mountain to the different places which I viſited, though almoſt all diſtant from its baſe 15, 20, and 30 leagues.

As certain winds occaſion rain, while others occaſion drought, it will be neceſſary that I ſhould mention their meteorological effects, to be able to judge of the influence which they have on earthquakes and volcanoes.

19. It is well known that when two contiguous parts of the atmoſphere are of different denſities, the denſeſt part, which conſequently has the greateſt elaſticity, moves towards that which has the leſs, forming a current of air, which extends towards the ſide where there is the leaſt reſiſtance.

20. The current of the trade winds at ſea follows the courſe of the ſun; but it changes its direction near iſlands and continents.

21. The eaſtern part of the iſland of St. Domingo receives the trade wind without changing its direction.

22. The whole north coaſt of the ſame iſland changes the direction of the trade wind; and, inſtead of ſuffering it to continue its natural courſe to the weſt, attracts it, and makes it incline towards the land, to form a compound current proceeding from north-eaſt to ſouth-weſt.

23. The weſtern part, from Port à Piment to Port au Prince, attracts the air of the weſt towards the eaſt, in a direction altogether contrary to the trade wind, and to that of the eaſtern part, or of Samana.

24. The ſouthern part of the ſame iſland attracts the trade wind, and makes it change its natural direction from eaſt to weſt, to form a current of air proceeding from ſouth-eaſt to north-weſt.

25. All changes which take place in regard to the current of the trade winds ariſe, in all probability, from the ſun darting his rays in the ſolid parts of the iſland, which imbibe them in the ratio of their denſities, and even of their colours.

26. All

26. All theſe warm bodies dilate the ambient air of the atmoſphere by which they are covered.

27. The vapours of the ſea, being attracted around all the coaſts towards the centre of the iſland, according to the 20th, 21ſt, 22d, 23d, and 24th obſervations, produce an aſcent of the clouds much more conſiderable on land than at ſea. This elevation of the vapours appears in a very remarkable manner in all the iſlands of the torrid zone, when one is at the diſtance of ſix or eight leagues from them, and even more. Experienced mariners employ this indication ſometimes to determine the proximity of land.

28. All the different breezes here mentioned change their direction in the night-time: they decreaſe in velocity from ſun-ſet to 9, 10, or 11 at night. After which a calm takes place for ſome moments; the land wind then prevails during the whole night, in conſequence of the condenſation of the vapours of the atmoſphere, which being cooled, and acquiring gravity by the abſence of the ſun, in their deſcent expel the ambient air, which they preſs on all ſides from the centre towards the circumference, that is to ſay, from the interior of the iſland towards the ſea: this is what is called the land breeze.

29. Of all the breezes here mentioned, the eaſt and north winds are more frequent in the countries of the torrid zone than thoſe of the weſt and ſouth, except in the diſtricts mentioned in the 23d and 24th obſervations.

30. The mountains form ſo many dikes that oppoſe the courſe of the clouds which are there accumulated, and, being condenſed, fall down in rain on the ſides and at the bottom of the mountains.

31. The plains on the ſide where the clouds are accumulated are fertile, and thoſe on the oppoſite ſide are dry. Theſe mountains therefore divide theſe countries into two different climates.

32. Earthquakes are much more frequent in arid countries than in diſtricts watered by rain; becauſe, in all probability, the vapours of the interior part of the country are there collected, and proceed thither more than to thoſe where the rains render the earth compact.

33. The faces of mountains which look towards the north and the eaſt, are more cooled by the rains than thoſe which look towards the ſouth and weſt.

34. In the Spaniſh plains, ſituated in the centre of Saint Domingo, there are ſeveral inſulated mountains, which, notwithſtanding their ſmall extent, exhibit the different temperatures indicated around their ſides by ſhades of verdure ana-

logous to the coolneſs or dryneſs occaſioned by theſe different poſitions.

35. The mountains of countries ſubject to great rains abound, in general, with very hard rocks which riſe abruptly; the places which are leſs ſteep are covered with compact, red, black, and fat earth, which produces vigorous trees of a dark green foliage, loaded with creeping plants, and ſurrounded by herbaceous ſhrubs, and heaps of manure ariſing from the rotten leaves and trunks.

36. The mountains of arid countries, being compoſed of a great deal of light earth, are rounded at the ſummits; they have in general a more gentle declivity than thoſe of rainy countries, and, when too rapid, fragments of them crumble down: the trees which grow on theſe mountains produce very hard timber, and ſcarcely any creeping plants are found near them.

37. The earth of mountains watered by rains being compact, as obſerved in the 35th obſervation, does not permit the rain water to penetrate them; it trickles down the ſurface, and flows into the ravines, at the ſame time that it falls from the heavens.

38. The mountains in arid countries are in general covered with calcareous earth or chalk, and the fragments of other tender ſtones mixed with the powder of dried vegetables, which altogether form a ſpongy light earth. This earth is eaſily penetrated by rain water, and it retains it in the boſom of the mountains or of the eminences by which they are ſurrounded. This water eſcapes merely by a ſlow filtration, which produces continual ſprings that maintain the ſtreams in the ravines; and theſe, in their turn, ſupply the rivers that fall into the ſea. It is for this reaſon that thoſe parts of St. Domingo towards the weſt have more ravines and running ſtreams than the northern and eaſtern parts of the iſland. The caſe is the ſame in the Great Antilles.

39. The calcareous ſtones found on the mountains of the weſt part of St. Domingo exhibit the empty neſts of the polypes by which they were formed. Theſe foſſils are found in all the mountains of St. Marc and the Gonaives in ſuch abundance, that when broken with a large hammer it is more difficult to find any without the impreſſion of a madrepore than thoſe from which it is effaced *.

40. In

* To aſcertain with certainty whether theſe foſſils have been left in conſequence of the ſurface of the ſea becoming lower, I cauſed a well to be dug at the bottom of a rock on a plantation which I had oppoſite to the Cape Town, in order to erect there a machine I had conſtructed at a great expenſe,

40. In the north-eaſt part of St. Domingo it is obſerved that the calcareous ſtones of the mountains are very hard, and exhibit no traces of their origin: they muſt be poliſhed before it is poſſible to diſtinguiſh in them, as in other marbles of the ſame kind, corals, the polypiers of madrepores, and the other characteriſtic marks of calcareous ſtones.

41. The diſtricts which front the weſt are thoſe where volcanoes manifeſt themſelves in preference to others.

42. Next to the diſtricts which front the weſt, thoſe which look towards the ſouth are thoſe where ſubterranean fires oftenеſt produce volcanoes.

43. Extenſive diſtricts which look directly to the eaſt have no volcanoes, unleſs a continent or large iſland be to the eaſt of the place where they might manifeſt themſelves.

44. The volcanoes of Sicily, according to ſeveral obſervations made on different eruptions, ſeem to be only ramifications of Veſuvius ſituated to the north-eaſt of Ætna, Lipari, and the other ſpiracles in the weſtern part of the kingdom of Naples.

45. There are only two or three volcanoes in all thoſe parts of the globe which are known, that incline towards the ſouth-eaſt: they are thoſe of the iſland of Bourbon and of Kamtſchatka.

46. In no part of the known world is there a volcano in a ſituation that looks towards the north.

47. Of the great number of volcanoes found in ſituations looking to the weſt, there are 42 in the courſe of 400 leagues

expenſe, that poſterity might know it in a poſitive manner; but, being in haſte to ſet out for France, in conſequence of my being appointed a deputy of the colony, and as my conſtituents pretended that the aſſembly of the ſtates-general would be ended before my arrival if I did not ſpeedily depart, I was obliged to leave this machine in May 1789, without being able to finiſh it. It conſiſted chiefly of various movements which turned four large copper cylinders, each ſix feet in height, with their floats and other apparatus, according to the form of my meteorographs, in order to mark the height of the tides each day, every month, and every lunar year for a century.

The riſing and falling of the ſea was to make the water enter and flow out by a canal a foot in breadth: a ſmall aperture of ſix lines only in diameter placed at the bottom of the well was to receive the water of this canal ſo as to ſuffer it to riſe and fall only in a very gentle manner. A large float of copper hermetically ſealed was to ſerve as a moving power to wind up this clock each tide by means of a kind of ſcapement.

But unfortunately at the time the Cape was burnt my houſe was plundered, and all the copper, iron, and ſteel work of this machine were carried away; ſo that I have been told no veſtiges of it are left. A large cheſt full of manuſcripts, the fruit of thirty years labour in the arts and ſciences, uſeful to the colony, ſhared the ſame fate.

in the Cordilleras, which extend along the South ſea from Chili to Panama.

48. There are reckoned to be 30 or 35 volcanoes on the coaſt of New Spain which looks towards the ſouth-weſt on the ſide of the Pacific ocean.

49. The volcanoes ſituated on lakes or deep gulphs, and which have a narrow mouth, manifeſt themſelves ſometimes differently. That of the lake of Nicaragua is ſituated on an iſland to the eaſt of the iſthmus which ſeparates that lake from the South ſea, or to the north-weſt of the other parts of the continent.

50. Volcanoes are more violent after long and heavy rains, than after and during the time of drought.

51. When rain falls in an equal quantity on all the parts of a mountain, that which falls on the ſummit muſt naturally run down and penetrate very little into the earth; while the ſides receive not only that of the heavens, but that alſo which comes from the ſummit.

52. The ſides of a mountain become plane in proportion as they become lower, and to their diſtance from the ſummit.

53. The loweſt parts of the ſides of a mountain muſt neceſſarily imbibe more water than the higher parts. The quantity imbibed muſt be greater as the inclination of the ſides decreaſes in approaching the baſe.

54. In the interior part of ſteep mountains there muſt be places to which the water cannot penetrate. Theſe places, in all probability, are thoſe which approach neareſt to the vertical line, proceeding from the ſummit and terminating perpendicularly towards the middle of the baſe.

55. If the filtration of rain water through the earth on the ſides of mountains ſtops up the pores, and if the ſubterranean vapours do not find room to eſcape through theſe ſides, they muſt proceed towards thoſe parts which approach the vertical line drawn from the ſummit, which is the place where they can find a paſſage by ſoaking through the pores of the dry earth which muſt be there found.

56. If gas and ſubterranean fires are thrown towards the axis of the mountain by the intermiſſion of waters which ſtop up their other paſſages, the elaſticity of theſe fluids accumulated at the centre muſt neceſſarily increaſe. It appears that, from this obſervation, we may deduce the reaſon why volcanoes often break out after long continued rains.

57. Whether it be the decompoſition of water, or any other cauſe, that makes the fire of volcanoes be diſengaged, this fire muſt neceſſarily, as already ſaid, purſue that route where it

it finds the leaſt reſiſtance to its free paſſage. It appears, then, that this route muſt be that where the waters have not ſtopped up the pores of the earth. It is for this reaſon, probably, that volcanoes manifeſt themſelves at the ſummits of mountains.

58. The crevices of volcanic mountains incline oftener from their centre towards the weſt or the ſouth than towards the other ſides. It is always towards thoſe parts that volcanoes throw out their lava. The reaſon of this ſeems to be explained by the 31ſt, 32d, 33d, 34th, 35th, and 36th obſervations, and in particular by the 37th and 38th.

59. The moſt violent volcanoes almoſt always manifeſt themſelves in the higheſt mountains.

60. It would appear that the ice on the ſummits of theſe mountains ought to ſecure the earth found there from being penetrated by the ſnow water which the temperature of the air may ſometimes diſſolve; and, as this temperature acts only on the ſurface of the ſnow, the ice below ought to throw off that which is melted towards the ſides where the declivity is, and by theſe means prevent the water from penetrating the earth of the ſummit to moiſten that at the centre of the mountain.

61. Volcanic mountains which are not very high, or which are not ſituated agreeably to the general poſition to which ſubterranean fires ſeem to proceed, have in general only weak volcanoes, which often in the end become extinct.

62. Many of the extinguiſhed craters have become lakes, or reſervoirs of rain water, at the ſummit of mountains; ſprings commonly manifeſt themſelves around their ſides, and ſerve to fertilize the lands at their bottom.

63. Hiſtory makes mention of many lakes which have been formed by mountains being ſwallowed up: all countries ſubject to earthquakes have a great many of them.

64. The earth has been ſeen to ſwell up ſo as almoſt to form a hemiſpherical mountain, and to burſt ſuddenly, with a horrid exploſion, and leave nothing but a lake in the place.

65. Many iſlands have emerged from the ſea: ſome of theſe have exiſted for a long time, others have gradually diſappeared; and ſome, in conſequence of a continual undulation, occaſioned by ſubterranean fire, have diſappeared after being ſlowly conſumed.

It is here proper to obſerve, that all theſe events take place in continents or iſlands the poſitions of which are analogous to thoſe mentioned in the 1ſt, 3d, and 9th obſervations.

Though this memoir is only a very ſhort extract from a manuſcript loſt during the troubles of the Cape, I think it my

my duty to publiſh the moſt ſtriking parts of that work which my memory has enabled me to preſerve. It is poſſible that among theſe obſervations there are ſome particulars which require to be rectified: but I ſhall leave to thoſe who may be deſirous of employing their time on ſimilar reſearches to examine them with attention, and to increaſe the number ſo as to form a complete work founded upon facts.

LVIII. *Obſervations on an Optical Phænomenon produced by Moon-light in the Vapours of the Atmoſphere. By Profeſſor* WREDE*.

ABOUT fifteen minutes paſt ten at night on the 14th of December 1800, I obſerved above the horizon of Joachimsthal, in the Ukermark, a very ſingular kind of reflection of the rays of the moon's light, and ſuch as I had never before obſerved. The temperature of the air, in conſequence of a thaw, was extremely moderate, and towards the ſouth and weſt the ſky was perfectly ſerene, ſo that ſtars from the firſt to the third magnitude, notwithſtanding the great ſplendour of the moon's light, could be clearly diſtinguiſhed on the blue ground of the atmoſphere. Towards the eaſt and north, however, it had a milk white colour, and there appeared near the horizon in the north-eaſt quarter a few ſtreaked clouds, which the common people generally conſider as an indication of wind. But theſe as well as the other cloudy parts were ſo tranſparent that ſeveral of the circumpolar ſtars, ſuch as the Great and Leſs Bear, Cepheus, and Caſſiopeia, could be obſerved. The lower parts of the atmoſphere, as well as thoſe ſtrata in which the above clouds floated, appeared on the firſt view to be entirely at reſt. The moon ſeemed to be nearly in the weſt-ſouth-weſt quarter, and was ſo bright that the ſpots of her illuminated ſide could be eaſily diſtinguiſhed. In regard to her aſtronomical poſition ſhe was near the ſtar π in the eaſtern band of the Fiſh, a little to the weſt of Mars, and much further from Jupiter. In this poſition ſhe was ſurrounded by two very eccentric rings, the ſmaller of which, being concentric, had the form of an ellipſe; but the larger was circular, as repreſented in the annexed engraving, (Plate VIII.)—The principal axis of the ellipſis lay ſouth and north, and the conjugate axis eaſt and weſt. It was interſected in the former direction by the eccentric circle,

* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. iii. 1801.

which

which at the ſame time paſſed apparently through the middle of the moon, and its centre lay to the eaſtward in the zenith of the obſerver near Capella in Auriga. The extent of this large circular ring, the apparent thickneſs of which was ſcarcely equal to the half of the moon's diameter, covered about the following places of the heavens: The band of the Fiſh, the head of Andromeda, a part of Frederick's Glory and the Dragon; the feet of the Leſſer Bear, Alioth in the tail of the Great Bear, the Small Lion, and the neck of the Large Lion, ſo that Regulus ſtood about two or three of the moon's diameters without the circle. It then proceeded between the Crab and the Water Serpent, above the Unicorn, under Orion's belt, above the Brandenburgh-Sceptre, Eridanus, George's Harp, and the Whale's Back, to the band of the Fiſh. The ellipſe extended, according to its greater diameter, from the ſtar ☉ to the northern arm of Andromeda, to the ſtar τ in the Whale, and then ran eaſt above the tail of Aries, ſo that Mars was included by it near δ of that conſtellation: on the weſt ſide it extended nearly to the equinoctial colure; on the ſouthern and northern edges the ellipſis appeared to be double, and ſometimes triple. There were ſeen here pale ſhining rings in pairs, one pair of which had a greater tranſverſe axis, and the other a ſmaller, than the principal ellipſis. The latter was the caſe, in particular, towards the end of the phænomenon. It ſometimes appeared as if the two exterior rings formed a particular ellipſis, which was concentric with the principal one, but which had a ſmaller conjugate axis, and interſected the bright, luminous, elliptical ring in four places. Where the latter was touched by the circular ring there appeared two ſtriking paraſelena, the northern one of which, about eleven o'clock, exhibited a few faint priſmatic colours; but the ſouthern one did not diſperſe the light in the ſame manner, only that at its circumference it had a ſomewhat yellowiſh white appearance. The points where the falſe ellipſis interſected the large circular ring, did not exhibit the ſmalleſt appearance of priſmatic refraction. The proper elliptical ring from the moon towards the north-weſt was very ill defined, and ſcarcely uninterrupted; and this was the caſe alſo with the large ring towards the neighbourhood of the Unicorn, where there were fewer vapours, and where the ſky towards the horizon was perfectly ſerene. In other reſpects, both rings appeared very diſtinct, and of a white colour; but this character was loſt by the large one within the ellipſis, and the nearer it approached the moon, which overpowered and obſcured it by her ſplendour. At this place, alſo, its thickneſs appeared to be as great as in the neighbour-

hood

hood of the Unicorn: in other places it was quite uniform. The elliptical ring towards the eaſt exhibited a very white and thicker part, which became deranged, and no longer coincided with the ellipſe the nearer the phænomenon approached to its end. This happened a few minutes paſt eleven o'clock, after both the rings had loſt their colour, and when the moon began to be ſurrounded by a yellow nimbus. At twenty minutes paſt eleven both the circle and ellipſe had diſappeared; but the nimbus around the moon had ſo increaſed, that, according to the eye, it was equal to ſix diameters of the moon, or about three degrees. The vapours in the atmoſphere were at this time no longer diſtributed in an uniform manner, but thrown together in maſſes of different denſities, which formed regular clouds, and in which ſome motion was obſerved. Neither ſnow nor rain, however, followed, but the weather as well as the temperature of the atmoſphere continued the ſame. About eleven o'clock the following evening a little cold rain fell; but the drops were exceedingly fine, and it did not long continue.

Having finiſhed the deſcription of this phænomenon, it might naturally be expected that I ſhould give ſome explanation of the cauſes which produced it; but I have not ſufficient confidence in my own abilities to undertake ſo difficult a taſk. As far as can be deduced from the principles of optics and the doctrine of light, it may be aſcribed to ſome inſulated clouds of different thickneſs and denſity, or to ſeveral ſtrata of clouds lying near and above each other. The phænomenon of the paraſelena, where both rings apparently interſected each other, gives us reaſon to conjecture that they actually cut each other; and that therefore we may admit as the cauſe the firſt caſe, that is to ſay, one ſingle ſtratum of clouds of unequal thickneſs and denſity. It is not improbable that, in regard to the elliptical ring, the caſe was the ſame as with the lenticular elevations in common window glaſs, which diſtort objects, extend them in length, and give to round things an elliptical form. But, in my opinion, it is more difficult to explain why the larger ring was perfectly circular, and appeared ſo eccentric; why the image of the moon lay at the end of the radius in the periphery. It is very probable, as has been already remarked by various obſervers, that during this phænomenon there were in the thick vapours of the atmoſphere ſeveral hollow cones or funnels which, bordering on the baſis of the image, formed ſeveral rings; for the latter occaſioned, in the places of interſection, an appearance of the priſmatic colours. Should this conjecture be agreeable to truth, the higher geometry, and par-

ticularly the doctrine of conic ſections, might be employed for explaining ſimilar phænomena. But reſpecting this point time muſt determine.

But that the above obſerved phænomenon may in ſome meaſure be uſeful to meteorologiſts, I ſhall here add a few remarks. If we eſtimate the tranſverſe diameter of the ellipſe at 60°, and the diameter of the circular ring at more than 120°, it muſt be admitted that the whole phænomenon was at no great height in the atmoſphere, elſe it muſt have comprehended fewer of the conſtellations: the vapours in which it appeared floated therefore in a very low ſituation, and near the obſerver's horizon. It thence follows, that this phænomenon could be viſible to no other perſon, unleſs he had the ſame ſtratum of clouds over him, and beheld the moon through it; and it is probable that the phænomenon might have remained unnoticed had the obſerver been a mile towards one ſide from the poſition in which he then ſtood. We may admit, not without reaſon, that all ſuch circles with which the ſun and moon are ſometimes ſurrounded are not at a very conſiderable diſtance from the ſurface of the earth, and therefore are not viſible in a greater extent than 40 or 50 miles. If the higheſt clouds over the ſea can be ſeen only at the diſtance of about 20 geographical miles above the horizon, how much more muſt a phænomenon which takes place in far lower diſtricts of the atmoſphere eſcape the notice of different obſervers whoſe poſitions are at a great diſtance from each other, and particularly when its viſibility depends on the angle under which an object muſt be viewed to obſerve that peculiar image which is produced! We never ſee the rainbow in a cloud, or rather in the falling drops of rain, when the ſun is either too high or too low. In the high Alps of Swiſſerland it ſometimes happens, that inſtead of a bow there is ſeen a complete circle of the priſmatic colours in the drops precipitated from the atmoſphere. In the ſame manner, the viſibility of various other phænomena in the atmoſphere depends on the point of ſight which is favourable for the obſervation. We therefore ought not to aſſert that ſuch or ſuch phænomena ſeldom occur; we ought rather to ſay that they have been ſeen by individual obſervers, and in particular geographical points of ſight.

I ſhall embrace this opportunity of making ſome obſervations on an aſſertion which appeared in one of the public journals of laſt year. Speaking of a rainbow produced by moonlight, which was obſerved on the 5th of September 1800 from the obſervatory of Göttingen, the account concluded

with

with aſſerting that this very rare phænomenon had been obſerved ſince the period of Ariſtotle only eleven times, and in Germany only once, by Weidler, in 1719. I can, however, produce two inſtances of a lunar rainbow ſeen in Germany. In the year 1799 or 1798 I obſerved in Pomerania, at a village in the neighbourhood of Wollin, a rainbow of this kind, even though the moon was not full. If my memory does not fail me, it was then a mild harveſt evening, and at moſt only eight o'clock. The ſky was pretty clear, and the moon was about 30 or 40 degrees above the horizon towards the eaſt. Towards the weſt I obſerved a cloud at about the ſame height, and in this was ſeen the rainbow, the colours of which were exceedingly lively. Its ends only were pale, and very faint. It was larger than a quadrant, but did not form a complete ſemi-circle. The livelineſs of the colours did not long continue after it was firſt obſerved; for a weſt wind drove the cloud before it, and a ſmall ſhower fell. In the month of October 1798, about eleven at night, I ſaw a ſecond lunar rainbow, during a ſhower, in a field of Joachimſthal. The moon was in the eaſtern quarter, and at a conſiderable elevation. The extent of this rainbow was about 180 degrees, and it ſtood with both its extremities on the ground. Its colours, however, were exceedingly faint; but the refraction was ſo apparent, that a ſecond pale rainbow was obſerved near to it. I have mentioned theſe two obſervations to ſhow that it is improper to ſay that theſe phænomena ſeldom occur; we ought only to ſay that they have been ſeldom delineated and made known. If the friends of natural philoſophy could make obſervations in every place where ſuch phænomena occur, or were thoſe ſeen by accidental obſervers, who often, through indifference, conſider ſuch things as hardly worth notice, communicated to them, there is no doubt that my aſſertion would be confirmed.

LIX. *Note upon a peculiar Vegetable Principle contained in Coffee. By* RICHARD CHENEVIX, *Eſq. F. R. S. M. R. I. A.**

IN a veſſel calculated to confine the vapour of water, I heated a conſiderable portion of that liquid upon about a pound of raw coffee imported directly from Martinico, and of the qualities of which I was well aſſured. I then filtered the liquor, and reduced it nearly to dryneſs, in a glaſs eva-

* Communicated by the Author.

porating

porating diſh, at a gentle heat. By this means I obtained a ſmall quantity of a clear yellow reſiduum like the moſt tranſparent horn, and of the conſiſtence of honey. This reſiduum did not deliqueſce, or ſeem to be ſubject to change, by expoſure to the atmoſphere. It was ſoluble in alcohol. It did not manifeſt either acid or alkaline properties. By ſome experiments I perceived it to be a ſubſtance differing eſſentially from all the vegetable principles with which I was acquainted; and, finding that I could obtain it pure by the method which Prouſt uſed to procure tannin, I proceeded in the following manner:

I poured a ſolution of muriate of tin into ſome water which had been made to boil upon coffee, and obtained a precipitate, which I collected upon a filter, and waſhed. I then put it into water, and cauſed a current of ſulphuretted hydrogen gas to paſs ſlowly through the liquor. By this proceſs the oxide of tin combined with the ſulphuretted hydrogen gas; and the ſubſtance originally contained in the coffee, but which, as I ſhall immediately ſhow, had combined with the metallic oxide, was diſengaged, and remained in the liquor; while the hydrogenized ſulphuret of tin was precipitated. It then remained only to evaporate the liquor to obtain the vegetable principle. In this ſtate it exhibited nearly the ſame appearance as before it had been combined with the oxide of tin; but ſeemed of a lighter colour, and more clear and tranſparent; being freed, as I ſuppoſe, from all extractive or other matter.

Imagining it now to be ſufficiently pure, I diſſolved it in a very ſmall proportion of water, and examined it chemically.

The ſolution was of a bright horn colour, and had a bitter taſte, though not unpleaſant. It was neither acid nor alkaline.

Solutions of potaſh, of ſoda, or of ammonia, poured into the liquor, changed its colour to a bright garnet red.

Nitric acid produced a ſimilar effect.

Very concentrate ſolutions of the alkaline carbonates did not cauſe a precipitate, as in a ſolution of tannin.

Sulphuric acid became of a dirty brown colour with the ſolution; but no other change was apparent.

With muriatic, phoſphoric, and the vegetable acids, there was no change, even of colour.

With muriate of gold, of platina, of copper, there was not any change, but what would naturally reſult from a mixture of the colours of both liquors.

With any ſolution of iron, in which there was not exceſs

of

of acid, the liquor paſſed to a beautiful green; and, if it was concentrate, there was a green precipitate. Salts formed of the red oxide of iron ſucceeded the beſt; and the reciprocal action of this principle and iron is almoſt as delicate as that of either gallic acid or tannin, and iron.

With muriate of tin, there was a very abundant yellowiſh precipitate, which was a combination of the new vegetable principle, with oxide of tin. Both this precipitate and that with iron are ſoluble in all the acids, and the liquors loſe their colour.

Lime water did not cauſe any precipitate in this liquor, nor did ſtrontia water. Barytes water gave a fawn-coloured precipitate. With lime water, tannin gave a blueiſh green precipitate; and nearly the ſame with ſtrontia water, as alſo with barytes water.

A ſolution of animal gelatine did not give any precipitate with this vegetable principle. The effect of tannin upon gelatine is well known.

By the above experiments it appears that this principle bears ſufficient characters to diſtinguiſh it from tannin, or any other vegetable principle with which we are acquainted. The only property which it poſſeſſes in common with tannin, is its affinity for oxide of tin; while it is clearly diſtinct from tannin in every other reſpect.

It is evident that coffee before it is roaſted does not contain tannin. A ſolution of gelatine poured into a decoction of roaſted coffee, gives, however, an immediate precipitate; and this precipitate is the combination of tannin with gelatine. Meſſrs. Prouſt, Seguin, and Davy, have obſerved that heat develops the tanning principle in many vegetables. In a commercial point of view, it would be eſſential to examine whether thoſe vegetables are not ſuch as, before being heated, contain this new principle. Although I did not perceive that the principle, when inſulated from the entire vegetable, was converted by heat into tannin, yet the preſence of the other component parts of the vegetable may influence the diſtribution of elements in ſuch a manner as to produce combinations different from what the ſeparate principles would afford.

I have not had an opportunity to look for this new principle in other vegetables.

LX. *Biographical Account of* JOSEPH DE BEAUCHAMP, *the Aſtronomer. By* DE LALANDE.

JOSEPH DE BEAUCHAMP was born at Vezoul on the 29th of June 1752. In 1767 he entered into the order of the Bernardins, of which his uncle Mirandeau had a regular abbey. The latter having been appointed biſhop of Babylon in 1774, deſtined his nephew to participate with him in his functions, and ſent for him to Paris that he might fit himſelf for that purpoſe by ſtudying the Arabic, theology and medals, of which the biſhop was exceedingly fond. Young Beauchamp, who had a decided taſte for the ſciences, attended the College de France, and made a rapid progreſs. I ſeconded his turn for aſtronomy by ſhowing him how uſeful he could be to us in Aſia. In 1780 he made it his chief ſtudy, and was of great ſervice to us. Soon after he ſet out for the Eaſt, and on the 15th of September 1781 arrived at Aleppo with his uncle, who was not able to continue his journey. Young Beauchamp, therefore, proceeded alone to Bagdad to diſcharge the epiſcopal functions.

In 1781 I obtained from the miniſter of the marine inſtruments which I ſent to him, and which he employed with great advantage. An account of his journey from Aleppo to Bagdad was publiſhed in the *Journal des Savans* for 1784: his obſervations made at Bagdad, and ſome notices reſpecting the Turks and the Arabs, may be found in the ſame work *. In the month of January 1784 he ſet out for Baſſora: a chart of the courſe of the Tigris and the Euphrates from Diarbeker to the Perſian gulph, that is to ſay, for an extent of 300 leagues, which he conſtructed on that occaſion, is in my poſſeſſion; and I publiſhed a ſhort account of this journey in the *Journal des Savans* for 1785 †. The different volumes of this journal, as well as the Memoirs of the Academy, contain a great many obſervations made by Beauchamp; ſuch as the paſſage of Mercury over the ſun's diſk, May the 4th, 1786 ‡.

I have alſo a map of the country round Babylon which he conſtructed. He brought to the abbé Barthelemy drawings of monuments, inſcriptions, and medals, found at the antient Babylon, as well as Arabic manuſcripts. In his firſt journey, having embarked on board a ſmall Arabic veſſel, it was

* *Journal des Savans* 1784, p. 332 and 470.

† For the month of May, p. 246 and 285.

‡ *Journal des Savans* 1787, p. 361. For an account of the eſtabliſhment of his obſervatory ſee p. 301 and 498.

overtaken by a calm at ſea, and, being ſhort of water, he was expoſed to the ſcorching heat of the tropical ſun for forty-eight hours without a drop of water to cool his mouth; in conſequence of which he was ſeized with a violent fit of illneſs on his return to Baſſora.

In 1787 I induced him to undertake a journey to the Caſpian ſea, to ſettle the queſtion in regard to its ſituation, and to determine the longitudes in that part of Perſia, in regard to which there were from five to ſix degrees of uncertainty. The reſult of his obſervations I publiſhed in the Memoirs of the Academy for 1787. During this journey he was maltreated and plundered, and he was thrown into a fever which laſted eighteen months. He however conſtructed a chart of his route, which Baron von Zach has publiſhed. He obſerved an eclipſe of the moon at Caſbine on the 30th of June 1787: it was one of the moſt important ever obſerved. He returned from his Perſian voyage to Bagdad on the 14th of January 1787 *.

Obſervations of Mercury, which are ſo rare in France, were among thoſe things which I had particularly recommended to him; and he did more in this reſpect than all the European aſtronomers together, and than was ever done ſince the origin of aſtronomy: he ſaw Mercury nearer the ſun than was ever before obſerved. I publiſhed ſeveral of his obſervations in the Memoirs of the Academy; and to him I was indebted for the principal aſſiſtance I received in conſtructing the tables which I publiſhed of that planet. He obſerved alſo ſome ſtars which could not be ſeen at Paris; and his manuſcripts which he left to me will furniſh a ſupplement to the immenſe collection of ſtars I have publiſhed in the *Hiſtoire Celeſte*.

Baron von Zach, in his excellent journal, has made frequent uſe of Beauchamp's obſervations; and he cauſed to be engraved for it his map of Perſia. Beauchamp had undertaken a general review of the heavens to rectify the poſitions of the ſtars, and he had aſcertained that of ſeveral thouſands, when he learned that the department of the marine had withdrawn the annual gratification of 2000 livres which he enjoyed, and without which he could not ſubſiſt at Bagdad. He therefore left Bagdad on the 1ſt of December 1789, and arrived at Paris on the 3d of September 1790.

In 1791, and the following years, I did not ceaſe to ſolicit the miniſters of the National Aſſembly that he might be ſent

* An account of this journey to Perſia may be found in the *Journal des Savans* for January 1790, p. 726. For his notes on the Babylonian antiquities ſee the Journal for 1790, p. 796.

back to Bagdad to resume his observations. In the month of February 1795 I obtained from the Convention, through the means of Gregoire, an indemnification for him; and on that occasion Beauchamp said, "I see that you draw upon me a bill of exchange which I can pay only in Turkey. I shall therefore set out; but if any misfortune befalls me you must remember my devotion to you and to astronomy." He indeed quitted, with some degree of regret, a country and family which he loved, and by which he was beloved. But it was not till the 3d of March 1795 that I obtained, by the aid of C. Volney, his nomination to the consulship of Mascate, in Arabia. He came to Paris on the 27th of March, but was obliged to proceed to Italy to procure a ship bound to Constantinople: he encountered various impediments, and in the month of April 1796 was still in Italy. He at length set out, went to make some observations in the Archipelago, and did not arrive at Constantinople till the 22d of November 1796. Towards the end of May 1797 he undertook a journey from Constantinople to Trebisonde, which was of great importance to geography, as he rectified the charts of the Black sea, which were exceedingly erroneous. On the 9th of December the same year he returned from the Black sea, and made preparations for proceeding to Mascate; but the war rendered this voyage dangerous and useless.

In the month of March 1798 he was sent to Egypt till he could find an opportunity of proceeding to Mascate, and, towards the conclusion of that year, offered to Bonaparte to go as a negotiator for peace to Constantinople; but he was arrested as well as all the other French, and remained three years in confinement.

Being at length set at liberty, in consequence of the peace, he quitted Constantinople on the 23d of September 1801, though still indisposed, and had scarcely arrived at Nice when he expired on the 19th of November. A few days before, he had been appointed by the first consul commissary-general at Lisbon; and he lived long enough to learn that this distinguished mark of favour had been conferred upon him.

Few men have employed the short course of human life to so much purpose. Beauchamp possessed knowledge and merit of various kinds: the duties of religion were not neglected by this philosopher, and the congregation of the Propaganda at Rome testified its satisfaction with the zeal he had displayed in his apostolic functions. I requested his uncle to endeavour to procure for him the reversion of his bishopric, which he promised to do; but he died in 1798 in

the hoſpital of incurables, after having interfered, to no purpoſe, in the epiſcopal revolution.

Never was aſtronomer expoſed to ſo many hardſhips in making obſervations. By the exceſſive heat of Bagdad he was reduced ſometimes to a ſtate of the greateſt debility, and his laborious and dangerous journeys always brought on ſevere illneſs. He died a victim to his zeal; and it increaſes our regret, that the number of the martyrs to aſtronomy is already ſo great. [The annexed engraving repreſents Beauchamp in his Arabian dreſs.]

LXI. *Notices reſpecting New Books.*

Ausführliche Geſchichte der Theoretiſch Praktiſchen Uhrmacherkunſt, &c. A Hiſtory of Clock- and Watch-making, both Theoretical and Practical, ſince the earlieſt Method of dividing the Day to the End of the 18th Century. By I. H. Moriz Popee, 1801. 8vo. 564 pages.

[Concluded from p. 276.]

VIII. *FURTHER progreſs of the theoretico-practical part of clock- and watch-making till the end of the 18th century.*—In order to render the oſcillations of the large clocks iſochronous, Huyghens invented the cycloidal plate; and De la Hire aſſures us, that this cycloidal pendulum clock, though often compared with the motion of the fixed ſtars, did not differ in eight days but a few ſeconds from the mean motion of the ſun. But Huyghens himſelf ſoon found that the plate could not be made in a cycloidal form, and that the ſilken threads by which the pendulum was ſuſpended did not anſwer the purpoſe, but that a pliable ſpring could be uſed in their ſtead. He then invented, for the purpoſe of more perfect regulation, that ſingular pendulum called *pirouette*, which has a cruciform motion. This, however, was alſo rejected. About the end of the 17th century, Derham and Hook conceived the idea of making the pendulum ſwing in ſmall arches, and applying heavy lenticular weights. This method was adopted by Le Bon and De Rivaz at Paris, and by Clement in London, who alſo invented the ſo called Engliſh pallet. The author here gives an account of the diſcovery, that the pendulum does not every where vibrate with the ſame velocity, its vibrations being quicker near the pole and ſlower towards the equator; on which account, clocks that go well in one place alter their rate of going when brought near to the pole

pole or the equator. He also mentions the disadvantages attending the stopping of the balance-wheel in watches; and relates what Sully, Hook, Hautefeuille, and Du Tertre, individually did to obviate this stopping; and also the experiments of Le Roy, Tompion, Flameville, and Facio, in this respect. Berthoud taught the method of cutting the teeth in the balance-wheel mathematically. John Prier of London, and Samuel Anguilins, a Swede, made researches also on the same subject. The author mentions also Schott's proposal in regard to moderating pendulum clocks, as also those of Du Tertre and Le Roy. The recoil scapement, invented by Clement in 1680, was changed by Graham for a dead scapement, according to which the scapement-wheel at each fall of the pallet remains at rest. Graham also made the scapement in watches to rest by means of the cylinder and pallet-wheel; by which the friction was lessened, and the machinery rendered fitter for greater and easier vibrations. This scapement, however, requires oil, which is often hurtful. In order to lessen friction still more with the dead scapement, Berthoud improved an invention of Mudge, which consists in this, that the balance-wheel is not checked by the balance itself, but by a particular catch which the balance lets loose, by which means the balance continues its oscillations while the wheel is retained by the catch. The balance makes two vibrations, one forwards and another backwards, so that the tooth brought to rest is set free at the second vibration. This kind of scapement, which is a striking proof of human ingenuity, is called the *free*. The author here describes, in an interesting manner, the different kinds of free scapements invented by Magellans, Platier, Rendal, Robins, Grant, and Breguet. From the end of the 17th century, celebrated mathematicians employed themselves in endeavouring to discover a general theory for all the parts of watches, of which the author gives a particular account. Another difficulty to be overcome in regard to clocks and watches, was the influence which heat and cold has upon the expansion and contraction of metals, and which alters the motion of the whole. Graham is considered as the first who made experiments with a view of freeing the pendulum from such changes. He first conceived the idea of making pendulums of ebony, fir-wood, walnut-tree, &c.; but found that the moisture of the air produced other inconveniences. Experiments of the same kind were afterwards made by Magellan, Fontana, Ludlam, Schröter, Croftwaite, and Köhler. Graham next constructed a pendulum of an iron tube, which was filled to a certain height with quicksilver. Troughton,

inſtead of an iron tube filled with quickſilver, employed one of glaſs, having at the end a bulb like the tube of a thermometer. But ſtill happier was Graham's idea, to conſtruct a pendulum of ſeveral rods of different metals, ſo combined, that the expanſion of the one ſhould be fully compenſated by the contraction of the other; and this gave riſe to the gridiron pendulum. Before Graham, however, Short, Caſſini, and Ellicot had entertained a ſimilar idea of a compound pendulum; and the plan of a gridiron pendulum was firſt carried into execution by Harriſon in the year 1726. The author here gives a deſcription of the pendulums propoſed by Graham and Harriſon; and mentions the experiments of Berthoud, Grenier, and Syffert, for improving them. He ſpeaks alſo of Ellicot's lever pendulum, Grenier's lever pendulum, and the pendulum with the ſmall gridiron; as alſo Rivaz's tubular pendulum; and the ſimpleſt compenſation pendulum of all, invented by Faggot, a Swede, about the year 1740, and which Schmidt the watchmaker, of Stettin, has lately employed with much ingenuity. The higheſt degree of perfection, however, to which watch-making has attained, is in the conſtruction of the nautical time-keepers, for the invention of which large premiums were offered in England, France, Holland, and Spain. Huyghens and Sulley made attempts for this purpoſe, but they were not attended with ſucceſs; and the ingenious propoſals of Leibnitz were not found ſufficient. Harriſon's firſt time-keeper, which he preſented to the Royal Society in 1736, was regulated by balancing rods placed croſswiſe over each other, with circular ſprings at the ends, which reſted againſt two plates, which by the dilatation of the circular ſprings, in conſequence of heat, ſeparated, and on their contraction by cold approached each other. The friction alſo was leſſened, and the time-keeper was ſuſpended like the mariner's compaſs. In a voyage of twelve weeks, the error in going amounted only to 36 ſeconds. A ſecond one, conſtructed in 1749, which was ſmaller and more convenient, ſurpaſſed the firſt. To a third, conſtructed in 1753, he applied a balance with a ſpiral ſpring, and a compenſation rod of braſs and ſteel. In 1761 he conſtructed a fourth, which, on a voyage of experiment that laſted 81 days, erred only 1 minute 54½ ſeconds. A fifth, finiſhed in 1764, erred only 54 ſeconds in ſix weeks. Dr. Maſkelyne, however, to whom it was afterwards referred for trial, did not give ſo favourable a report of it. Berthoud and Le Roy made attempts alſo to conſtruct time-keepers; but the firſt were not ſucceſsful. In the year 1741 Le Roy's time-keeper, which in ſix weeks erred only half a degree, was preferred to that of Berthoud, which in the

ſame

ſame time erred 34 minutes 36 ſeconds. The author gives an account alſo of Rivaz's experiments. The time-keepers of Arnold, Rendal, and Mudge, were found to be exceedingly correct. Mudge conſtructed only three; the firſt of which was tried, in the year 1774, by Dr. Maſkelyne, Hornſby, Count Bruhl, Von Zach, and Campbel. The other two were tried by Dr. Maſkelyne in 1777. One of them, in the courſe of 93 days, was found to have exceeded the mean time by only 1 minute 1·8 ſecond. It ſhowed the longitude between London and Oxford within 1·6 ſecond. The balance vibrated altogether independent of the wheel-work, and always received from the moving power a new impulſe, which at each vibration was uniform. The balance had two ſpiral ſprings, which produced a very uniform action. Beſides this, the machine was furniſhed with a compenſation balance. In imitation of the nautical time-keepers, pocket chronometers were conſtructed. Theſe ſerve for determining the geographical longitude at land. In theſe the free ſcapement of Mudge is employed. Emery, an artiſt from Neufchatel, made the firſt, which, after a paſſage of four weeks, gave the longitude of St. John's, Newfoundland, correct within ſix ſeconds. Another, conſtructed by Mudge, in a voyage of 14 weeks, erred only 17 ſeconds. Mudge never made but two pocket chronometers. Arnold made above 900, of various conſtructions. His chronometers of the beſt kind, with gold caſes, coſt 120 guineas, with ſilver caſes, 100.

In the year 1500 the art of clock- and watch-making in Germany became a particular branch of buſineſs. The author here gives an hiſtorical ſketch of the origin and progreſs of watch-making in Swiſſerland from the year 1679 to the preſent time, particularly in Neufchatel and Valengin, which will be read with great intereſt. In both theſe diſtricts there are at preſent 3634 clock- and watch-makers. One Abraham Robert invented there an inſtrument which ſerves for adjuſting the teeth of the wheels and pinions. The ſame artiſt conceived the idea alſo of a dead ſcapement. Perrelet invented the inſtrument for placing wheels ſtraight; and Recorder watches, which wind themſelves up. The moſt celebrated watch-maker in Chaux de Fond, where there are above 400, is James Droz, who has obtained great celebrity by the conſtruction of ſome very curious automata.

XI. *Machines moved by clock-work.*—Under this head the author deſcribes, with great minuteneſs, all thoſe works moved by clock-work which are not employed for meaſuring time. The reader will here ſee, with pleaſure, how much can be produced by human ingenuity; but it is not poſſible to give

a proper idea of it in an extract. We shall only observe that the author here gives an account of planetariums; the most remarkable town clocks by Hahn, Mollinger, Dienel, and the works of Jacob, the celebrated Huttig, a weaver of Bunzlau, and of Droz; also the automata of Vaucanson, &c. together with waywisers, pedometers, and other objects of the like kind.

X. *Bibliography of clock- and watch-making.*—The author under this head mentions not merely the titles of books, but gives an extract of their contents with critical remarks.

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

THE following letter from Baron von Zach to the president Sir Joseph Banks, contains some interesting information respecting the planet Ceres Ferdinandea.

DEAR SIR, Seeberg Observatory near Gotha, Feb. 20, 1802.

I had the honour to send to you my observations of the new planet Ceres Ferdinandea made in January here. I take the liberty of sending the continuation made in February.

1802.	Mean Time in Seeberg.			Ap. Right Ascen. observed.			App. Dec. observed.		
Feb. 3	15^h	40′	35″ S.	188°	42′	13·05″	12°	40′	5″ N.
4	15	36	41·4	188	42	36·30			
5	15	32	45·1	188	42	30·15	12	50	25
9	15	16	43·7	188	38	3·90	13	14	18
19	14	34	46·7	187	58	27·90	14	20	3

Dr. Gauss has corrected his elliptical elements of the orbit upon my observations; here is what he has found since my last letter to you.

Epoch for the beginning of the year to the meridian of Seeberg - - - 77° 27′ 36·5″

Aphelion	both sidereal -	325	57	15·0
Node	both sidereal -	80	58	40·0
Greatest equation of the centre -		9	20	8·0
Inclination of the orbit - -		10	37	56·6

Logarithm of half axis major 0·4424742

Eccentricity of the orbit - 0·0814064

Mean diurnal heliocentric and tropical motion 769·7924

With these elements of the orbit, all the observations made by

by Mr. Piazzi in Palermo, from January 1, till February 11, 1801, agree perfectly well, and within a few seconds; and my obſervations are repreſented by them thus:

Seeberg obſerv.	R. A. calculated.	Differ.	Declin. calcul.	Differ.
1801, Dec. 7	178° 33′ 29·2″	− 1·4″		
1802, Jan. 11	186 45 47·6	− 2·3		
16	187 27 38·8	−14·4		
22	188 6 18·2	− 7·6		
25	188 20 37·2	− 2·0	11° 56′ 58·4″	+35·4″
26	188 24 37·0	−12·5		
28	188 31 25·7	−12·1	12 9 55·6	+14·3
29	188 34 14·1	− 4·0		
30	188 36 38·4	− 5·5	12 19 19·8	+19·1
31	188 38 38·3	− 7·1	12 24 15·3	
Feb. 3	188 42 7·8	− 5·2	12 39 53·6	−11·4

As theſe elements agree hitherto ſo well with the heavens, the following ephemeris, calculated upon them for the next month, will probably do the ſame; ſo I annex it here to point out to the Engliſh obſervers the place where they have to look for the Ceres.

Poſition of the Ceres for Midnight Mean Time in Seeberg Obſervatory.

1802.	R. A. in Degrees.	Decl. N.	R. A. in Time.
March 1	186° 41′	15° 30′	12ʰ 26′ 45″
4	186 11	15 50	12 24 45
7	185 39	16 10	12 22 36
10	185 5	16 29	12 20 18
13	184 28	16 47	12 17 53
16	183 51	17 4	12 15 24
19	183 13	17 19	12 12 50
22	182 34	17 33	12 10 15
25	181 55	17 44	12 7 40
28	181 17	17 54	12 5 7
31	189 39	18 1	12 2 37
April 3	180 3	18 6	12 0 12
6	178 29	18 10	11 57 54

This planet will come in oppoſition to the ſun, March 17, in the afternoon. At the ſame time this heavenly body will be

be in its greateſt proximity to the earth = 1,6025, and therefore the moſt favourable time to look for its ſatellites, if there are any, to meaſure its diameter, and to examine its nebuloſity. About this time the planet will alſo be in its greateſt geocentrical latitude = 17 9, and a little later ſhe will have her greateſt retrograde motion, about 13′ in right aſcenſion per day. The north declination will increaſe till the beginning of April, and about the ninth of the ſame month the motion in declination will commence to the ſouth. It appeared to me that the Ceres has ſome change of light. I imputed it at firſt to our hazy atmoſphere this winter; but Mr. Schröeter of Lilienthal, and Mr. Olbers of Bremen, ſent me word that they have obſerved the ſame, and they believe that it is the planet which is ſubject to ſuch changes of light. Mr. Herſchel will tell us beſt whether it is ſo. I have ſome hopes to find the planet in antient catalogues of ſtars. Mr. Melſier was very near it in the year 1779. The famous comet of that year ran juſt over the northern wing of Virgo, as now, and the new planet was not very far diſtant. If the comet had attained two months ſooner the completion of Virgo, Mr. Melſier muſt infallibly have obſerved the Ceres then, becauſe he determined all the little ſtars in the vicinity of the comet; the planet would have been in the way of the comet, and ſo, of courſe, he would have catched the little planet in 1799.

If my obſervations are acceptable to you, dear Sir, only a little hint, and I ſhall continue, with pleaſure, to give you further intelligence.

I am, with the greateſt eſteem and regard,
very reſpectfully, moſt honoured Sir,
Your obedient humble ſervant,
FRANCIS BARON DE ZACH,
Lieut. Col. and Director of Seeberg Obſervatory, near Gotha, Saxony.

On the 29th of April and the 6th of May, the reading of Count Bournon's paper on corundum was continued, and on the 13th of May was concluded. It furniſhes a complete and ſcientific mineralogical deſcription of that ſubſtance.

On the 6th and 13th of May there were read two papers communicated by Dr. Herſchel, giving an account of his obſervations during the late fine weather upon the two new celeſtial bodies diſcovered by M. Piazzi of Palermo, and Dr. Olbers of Bremen, and by them reſpectively called Ceres Ferdinandea, and Pallas. In our Magazine for March laſt, ſome account was given of a former paper by Dr. Herſchel, wherein he formed an eſtimate of the magnitude of Ceres by comparing

paring its apparent disc with that of the Georgian planet. It was then thought very extraordinary that the calculations, founded upon such a comparison, and upon the most probable distance of Ceres from the sun, made the real diameter of that newly-discovered celestial body certainly less than 5-8ths of the diameter of our moon. By Dr. Herschel's subsequent observations, however, detailed in the first of the two papers above mentioned, and which were made in much more favourable weather, the diameter of Ceres was found much smaller than what he inferred it to be from his former observations of the 11th of February. His later observations consisted chiefly in his taking repeated measures by means of his lamp-micrometer; an apparatus long ago contrived by Dr. Herschel, and published in the Philosophical Transactions, for estimating the extent of angles much smaller than a second of a degree. By similar observations he found the apparent diameter of Pallas to be less than that of Ceres; and from those apparent diameters, and the best data for the distances from the sun, Dr. Herschel has computed their real diameters to be about 160 miles for Ceres, and about 80 for Pallas; from which it appears, that out of the quantity of matter which the planet Mercury contains 73,839 such bodies as Pallas could be formed. The existence of such relatively minute bodies in the heavens belonging to the solar system, and moving according to the law of gravitation, is a circumstance which appears to us extremely interesting; the knowledge of which must still further distinguish the present æra of astronomy, which has already been rendered so illustrious by Dr. Herschel's own labours in the field of discovery.

In his paper of the 6th of May he gives an account of several observations of a small coma or haziness which surrounded both Ceres and Pallas, the appearance and extent of which seemed to vary according to the state of the air. An account is also given of the method he followed to satisfy himself that these two stars have no satellites belonging to them.

In the same paper Dr. Herschel points out the advantage of distinguishing such new celestial bodies from the planets and comets by some appropriate name which will admit of a full and precise definition. By reason of their differing so much from planets and comets as scarcely to be perceivable from minute fixed stars, even by very good telescopes, Doctor Herschel has adopted the term *asteroids* to denote them; but reserving the liberty of substituting afterwards any other appellation which he may think more apposite, and to which the precise and copious definition laid down in his paper may equally apply. He next remarks, that from the nature of

asteroids

aſteroids the diſcovery of ſuch bodies in the heavens neceſſarily requires a particular method of obſerving, which hitherto aſtronomers have but ſeldom purſued. On account of their minuteneſs, they ſo far lie beyond the power of good teleſcopes to diſtinguiſh them, that Dr. Herſchel in his five different reviews of the zodiac detected none of thoſe concealed objects; whereas, had they leſs reſembled very ſmall teleſcopic ſtars, he muſt have infallibly detected them. It is therefore, he ſays, only by a diligent and accurate attention to minute ſtars, in the view of diſcovering amongſt them *ſuch as may be in motion*, that aſteroids can ſucceſsfully be diſtinguiſhed from the multitudinous collection of teleſcopical ſtars which ſo much abound in the heavens. A ſearch of this kind, from its nature, cannot be carried on except in obſervatories furniſhed with fixed inſtruments, which, it is well known, make no part of Dr. Herſchel's apparatus. In the concluſion of his paper, honourable notice is taken of the aſſociation of twenty-four aſtronomers in Germany who have portioned out the zodiac into as many parts, in order more effectually to explore it. As this new method of ſearching, in the hands of Piazzi and Olbers, ſo quickly produced important diſcoveries, Doctor Herſchel thinks it highly probable that more celeſtial bodies of the claſs of aſteroids remain concealed, which may ſooner or later be found out in conſequence of their obſerved motions.

On the 20th, the reading of a paper, by Richard Chenevix, Eſq. on the chemical analyſis of corundum, was begun. The chemical facts delivered in this paper, with Count Bournon's mineralogical account of the ſame ſubſtance, already alluded to, will perfect our knowledge reſpecting corundum.

BRITISH MINERALOGICAL SOCIETY.

Deſcription of the Satin Spar, by A. Aikin.

The ſatin ſpar, characteriſtic ſpecimens of which have been preſented to the ſociety's cabinet by the late Mr. Mohr, and Mr. Richard Phillips, is a mineral as yet peculiar to the neighbourhood of Alſton Moor, in Cumberland, where it lies ſo near the ſurface as to be occaſionally thrown up by the plough.

Its colour is a pure white, ſometimes ſtained by an ochrey yellow tint of more or leſs intenſity.

It occurs in thin ſtrata from one to three or four inches thick, traverſed by thin irregular ſeams of pyrites parallel to the direction of the ſtratum.

Its luſtre is between pearly and ſatiny, conſiderably chatoyant.

Its

Its perpendicular fracture is ſtraight or ſlightly waved fibrous, diſcovering an obſcure fine ſtratification parallel to the direction of the whole ſtratum: hence the varying or chatoyant luſtre is obſerved only when moving it in the direction of the fibres. The croſs or parallel fracture requires a ſtronger blow to effect it than the former, and preſents a compact, broad, ſplintery ſurface.

When broken, it flies into flattened fibrous fragments.

In thin pieces it is ſemi-tranſparent.

Can juſt be ſcratched by the nail: is brittle: non-elaſtic: ſp. gr. 2·709 to 2·721. It may be poliſhed to almoſt a mirror-like ſurface, and is then exquiſitely beautiful.

Its proper place in a mineralogical arrangement ſeems to be between compact ſtalactite (*dichter kalkſinter*), and ſlaty ſpar (*ſchieferſpath*).

Analyſis of the Satin Spar, by Mr. H. Pepys junior.

Preliminary Experiments.

A piece of about 400 grains was expoſed to a red heat for two hours. It loſt near 1-4th of its weight, and was much altered in its appearance, having loſt its ſmooth feel. Upon immerſion in water, it imbibed it with a hiſſing ſound, became hot, fell to powder, and a portion of it was diſſolved. The ſolution was rendered turbid by carbonic acid. The undiſſolved portion effervеſced ſlightly with acids, and had obtained an hepatic ſmell from ſome adhering pyrites.

A portion of the crude ſpar, carefully freed from all adhering pyrites, diſſolved completely in nitric acid, with effervеſcence. This ſolution was examined with the following re-agents:

On adding ſulphuric acid to a portion of the ſolution, the two fluids became nearly ſolid, and much heat was extricated; proving the preſence of lime, ſtrontian, or barytes.

Strontian water gave a precipitate of a brown colour, which was again ſoluble in nitric acid.

Barytic water gave a precipitate of rather a darker hue than the ſtrontian: this was alſo ſoluble in the nitric acid.

Tincture of galls, pruſſiate of potaſh, and ammonia, gave ſlight traces of the preſence of iron.

A ſolution of ſulphate of ſoda gave the ſame granulated precipitate as with a ſolution of nitrate of lime.

100 grains of the picked ſatin ſpar were expoſed to the action of 200 grains of nitric acid of ſp. gr. 1·25 in a veſſel of a conical form furniſhed with a ſmall glaſs worm on the top, that, ſhould any fluid be carried up by the gaſeous product, it might be depoſited in its paſſage. This and the veſſel were

were previouſly weighed. A ſtrong efferveſcence enſued upon mixture: the gas produced was heavier than atmoſpheric air; extinguiſhed flame; produced a clouded precipitate in lime, ſtrontian, and barytic water. 200 grains of the acid being found not ſufficient for the ſolution of the ſubſtance, 100 grains more were carefully added: efferveſcence again took place, and more of the ſame gas was liberated. It was then expoſed to a heat of 200 degrees Fahrenheit. Upon cooling, it had loſt 46 grains.

250 grains of the moſt perfect part of the ſatin ſpar were not ſoluble in 500 grains of nitric acid of the ſpecific gravity 1·25: upon the addition of 250 grains more, there was obtained a ſolution nearly colourleſs. It was then diluted with diſtilled water, and evaporated to the conſiſtence of a ſtrong ſyrup, without exhibiting any ſymptoms of cryſtallization, though the evaporation was ſuſpended, and the ſolution ſet to cool, thrice during the proceſs. The concentrated ſolution became ſlightly yellow.

100 grains in ſolution in nitric acid, left to ſpontaneous evaporation in a warm, dry place, after ſtanding two months exhibited no cryſtals.

Analyſis.

A. *For the carbonic acid.*—250 grains of picked ſpar were diſſolved in 757 grains of nitric acid of the ſpecific gravity of 1·25: the gas produced was received over mercury. It changed the colour of tincture of litmus red, and was abſorbed within one-hundredth of the whole by barytic water, giving a copious precipitate, which was ſoluble, and efferveſced upon the addition of diluted acid. 119 grains were given off by this treatment, equal to 47·6 per cent.

B. *For the calcareous earth.*—The nitric ſolution A was diluted with an equal quantity of diſtilled water: upon the addition of diluted ſulphuric acid it became nearly ſolid: it was then further diluted and filtered. The precipitate, dried in a heat of 212°, weighed 433 grains. It was then brought to a red heat, after which it weighed 330 grains.

C. *For the iron.*—The ſolution B was then treated with cauſtic or pure ammonia, which gave a ſlight red florulent precipitate. This precipitate, being filtered and dried, weighed 3 grains, equal to about 0·3 of metallic iron.

D. The ſolution C was then evaporated, during which, previous to the formation of any cryſtals of ſulphate of ammonia, 7 grains of cryſtals of ſelenite were depoſited of a ſilky needle-formed appearance.

E. The ſelenite B and D, = 337 grains, were decompoſed by

by a solution of carbonate of potash. The carbonate of lime thus obtained, being separated by the filter, and exposed to ignition in a black-lead crucible in a furnace for two hours, gave 125·2 grains of lime.

Satin spar, 100 parts contain,

Carbonic acid, A, 47·6 Lime, B, D, E, 50·08	Carbonate of lime -	97·680
Iron, C - - - -		012
Loss (probably water of crystallization) -		2·308
		100·000

Having repeated the experiments with a very perfect piece of the spar, I was not, during the whole process, able to detect the smallest portion of iron. I did not, in this instance, decompose the selenite formed, as it rated when ignited at 49·5 per cent. of lime. The minute portion of iron detected in the first analysis must therefore have been produced from some small pyritic grains in the substance of the spar then employed. Its quantity, as may be observed, was only about a thousandth of the whole.

Remarks on the Affinities of Lime, Strontian, and Barytes.

Previous to the foregoing analysis it was necessary to institute a few experiments on the affinities of the above-mentioned substance: as the results may perhaps be useful to others, they are here subjoined.

A solution of nitrate of lime in distilled water, is partially, but not immediately, precipitated by a solution of caustic strontian. On joining the two, the mixture assumes a sizey appearance.

A precipitation takes place also in a solution of nitrate of lime, when a solution of caustic barytes is added. The precipitate falls down much sooner in this case, and in more considerable quantity, than when the solution of strontian is employed.

Nitrate of barytes in solution is immediately decomposed by pouring into it a solution of sulphate of soda, which produces a copious precipitate.

Nitrate of strontian in solution is also decomposed by sulphate of soda when a solution of the latter is poured into it, but much slower than the nitrated barytes.

A solution of nitrate of lime, upon having a solution of sulphate of soda added to it, does not instantly give a precipitate, but in a very short time after yields a granulated precipitate of a more consistent form than any of the before mentioned.

The

The ſatin ſpar has been uſed in ornamental jewellery, ſuch as aer pendents, rings, &c.; and from its wavy and chatoyant appearance, with the brilliant poliſh of which it is ſuſceptible, ſeems well calculated to be employed as a faſhionable appendage to the fair ſex.

SOCIETY OF ARTS, MANUFACTURES, AND COMMERCE, LONDON.

This Society had its annual diſtribution of premiums and bounties for improvements in agriculture, chemiſtry, the polite arts, manufactures, &c. on the 25th of May, his Grace the Duke of Norfolk in the chair. About forty candidates were honoured with gold or ſilver medals, the ſilver pallet, or pecuniary rewards for uſeful improvements, or as badges of meritorious exertions in the different branches of the fine arts.

The meeting was numerous and ſplendid, many ladies of the firſt rank and faſhion being preſent.

Our limits do not allow us to inſert at this time a liſt of the ſucceſsful candidates: we cannot, however, cloſe the preſent article without mentioning that this ſociety, to which the nation owes ſo much for bringing forward numerous valuable diſcoveries and improvements, was the firſt to award, ſome months ago, an honorary and pecuniary recompenſe to Mr. Henry Greathead, of South Shields, for his conſtruction of a life-boat, by which the lives of many perſons ſhipwrecked have already been preſerved.

The life-boat is now coming into general uſe; and, from the exertions making by the gentlemen at Lloyd's, who have devoted 2000l., and other patriotic individuals, who are contributing their money and exertions to the eſtabliſhment of them on every part of our coaſts, we have the pleaſing proſpect of many thouſands of valuable lives being preſerved to their families and to the community by this ineſtimable contrivance. The reward of the Society to Mr. Greathead was their gold medal! and fifty guineas.

IMPERIAL ACADEMY OF THE SEARCHERS INTO NATURE.

The following prize queſtions have been propoſed for the year 1803:

I. Germany produces a conſiderable quantity of umbelliferous plants endowed with medicinal properties, which are as yet unknown, or known but imperfectly or from conjecture. Only a few of theſe have been admitted into the new diſpenſatories, and into Murray's *Apparatus Medicamentorum*. The number

number of the umbelliferous plants growing wild in Germany may be reckoned to be about 115: of these, Ehrhart, in the seventh volume of his Collections, names forty-four as officinal, which he has extracted from different dispensatories: but there are many of them the virtue and effects of which are not sufficiently or accurately determined, which were adopted in the old dispensatories, but which are no longer considered as medicinal. Murray, on this account, makes the number of those which ought to be retained twenty-eight; a number still lessened in the dispensatories of other countries, but which comprehends species which, in general, may be accounted medicinal. The academy wishes, therefore, to have determined—Whether there are any of the German umbelliferous plants not yet admitted into the *Materia Medica* which possess considerable virtue in medicine? What these virtues are? In what diseases, and in what manner, these plants, or individual parts of them, can be used? Those who employ themselves on these questions must select at least five species besides those mentioned by Murray, in order to ascertain, by chemical and medical experiments, the uses and purposes to which they can be applied. It will be agreeable to the academy, if those authors who have furnished any hints towards these researches be pointed out; but the writers must not content themselves with mere quotations.

II. The phænomena which have been observed in the effects produced by the Voltaic pile afford the most pleasing prospect of future discoveries in chemistry, physiology, and medicine, and even give reason to expect some conclusions in regard to the obscure secret of life and organization. Though we are still too far from having discovered the grounds of these phænomena to entertain a hope that such conclusions can be soon obtained—as an attempt to arrange and to reduce into a systematic form the many and various observations which have been made since the invention of the above apparatus by the industry and spirit of research, and particularly of the German chemists, might contribute to bring us nearer to the object in view, the Society proposes as the subject of a prize a treatise on this discovery, and requires:

1st, That the author will collect all the observations made before the beginning of the year 1803, and deduce from them a series of principles systematically arranged, quoting the names of the observers, and also the works which contain an account of their observations.

2d, That he will make new experiments in regard to those

principles not fully proved, and, if possible, form from them some decisive conclusion.

3d, That in establishing these principles he will make decisive experiments in regard to the action of the Voltaic pile on unorganized bodies, on pure water, alkalies, acids, metals, &c.; then on organized bodies, and particularly on dead bodies (in promoting or preventing putrefaction); and on living, in regard to shocks, pricking sensation, taste, luminous appearance, &c.

4th, That the author will attend in particular to the supposed identity of the galvanic power and electricity; and will mention and give an opinion on the explanations of the phænomenon offered by others, and particularly on those of M. Ritter. It will be agreeable also to the academy if the author will make experiments and observations of his own to confirm or refute the hypothesis of M. Ritter; and if he will employ Lavoisier's theory to explain these phænomena.

5th, That he will apply the principles and conclusions in the 1st, 2d, 3d, and 4th articles, to the practice of physic in particular; and endeavour to determine theoretically how far, and in what manner, the use of the Voltaic pile in medicine may be salutary? That he will collect, arrange systematically, try, and give some opinion of the observations already made by Grapengiesser, Hagenbach, Huber, &c. That he will make experiments of his own on the subject, and describe their consequences, both negative and positive, with fidelity and without prejudice; and will always mention the mode of application, for example, the position and number of the metallic plates, and whether formed into one or more piles connected together, &c.

The papers, written in Latin, German, or French, according to the usual regulations, must be transmitted to the president of the academy, at Erlangen, before the 1st of October 1803.

The prize is a gold medal of the value of 25 ducats, and will be adjudged to the successful candidate on the 5th of January 1804.

SOCIETY OF THE SCIENCES AT FLUSHING.

Some years ago, a stone having been found at Domburg, in the island of Walcheren, containing an inscription in which mention is made of a certain female divinity named *Buronina*, the above Society have proposed the following questions:

Who was the goddess Buronina, who seems to have been worshipped by the antient inhabitants, or by foreigners who

reſided among them? Are there any traces of this deity ſtill remaining? The prize is a ſilver medal. The anſwers, written in Dutch, Latin, or French, muſt be tranſmitted, poſt paid, to A. Dryfhout, of Middleburg, before the 1ſt of January 1803.

LXIII. *Intelligence and Miſcellaneous Articles.*

ASTRONOMY.

IN our laſt we mentioned Dr. Olbers's diſcovery of a new celeſtial body, and laid before our readers its obſerved places on the 28th, 29th, and 30th of March, and on the 1ſt of April.

In the ſciences, when a way is once opened to great and important diſcoveries, it ſometimes happens that they ſoon follow each other in ſucceſſion. It is now only about twenty years ſince the celebrated Dr. Herſchel made a diſcovery of which we have no previous inſtance in hiſtory, viz. of a new primary planet. On the 1ſt of January 1801 another was diſcovered by Piazzi; and a year after, a third new one has excited the curioſity of the public. On the 28th of March an account was publiſhed in the *Reichſazeiger*, No. 98, that Dr. Olbers, of Bremen, equally celebrated as a phyſician and an aſtronomer, had been ſo fortunate as to obſerve a moving ſtar in the north wing of Virgo, perfectly ſimilar in its form and light to the Ceres Ferdinandea; ſcarcely diſtinguiſhable from a fixed ſtar of the ſeventh magnitude; without any nebuloſity, and retrograde like Ceres, but increaſing much more in its northern declination. After obſerving this ſtar for three days, and aſcertaining that it had a motion peculiar to itſelf, he ſent notice of his diſcovery to Baron von Zach, of Gotha. The latter immediately found this ſmall ſtar, obſerved it on the 4th and 5th of April at the obſervatory of Seeberg, and found Dr. Olbers's obſervations confirmed in the fulleſt manner. This ſtar, to which he was inclined to give the name of *Pallas* in order to diſtinguiſh it from *Ceres*, appeared to him to be ſomewhat leſs bright than the latter. Mr. Schröter, of Lilienthal, who, in conſequence of the information communicated to him by Dr. Olbers, obſerved this ſtar with his 13 feet teleſcope, conſiders it to be ſomewhat larger and better defined than Ceres: its diameter he eſtimates at $4\frac{1}{2}$ ſeconds, whereas that of Ceres is only 4. But the obſervations made by Dr. Olbers and Baron von

Zach are ſtill too few in number to admit of any deciſive opinion being formed reſpecting the nature of this intereſting body. If it be conſidered as a comet, its regular motion and whole appearance oppoſe any idea of this kind; but there is every probability in favour of its being a planet, though its orbit muſt have an uncommonly great inclination to that of our earth. Dr. Olbers ventures to conſider it as a new primary planet of our ſyſtem, moving between Mars and Ceres in an orbit very much inclined to the ſun. Its period of revolution he makes three years, its mean diſtance from the ſun $2\frac{1}{10}$th of the diſtance of our earth from the ſun, and the place of the aſcending node 5^s 20°.

Mr. Walker, lecturer on aſtronomy, has obſerved this body, and deſcribes it as being of a pale red colour, very faint, and leſs brilliant than the Ceres, viſible with a definable diſc with a magnifying power of 100 times.

As the diſtance of this ſmall body from the earth is increaſing, and its light decreaſing, aſtronomers muſt haſten to make good obſervations if they are deſirous of determining its orbit in an accurate manner: it will otherwiſe be difficult to find it again.

The following notice from the aſtronomer Burckhardt contains ſome further uſeful information on this intereſting diſcovery:

"The ſtar diſcovered by Dr. Olbers on the 28th of March has ſo great a reſemblance to a planet, that it was natural to ſuppoſe its orbit to be ſomewhat eccentric. I have placed it ſucceſſively between the earth and Mars, between Mars and Piazzi's planet, and between the latter and Jupiter. I have employed the eccentricities of one and of two tenths (thoſe of Mars and Mercury), but neither of theſe ſuppoſitions has ſucceeded. It is eaſy to make a very ſmall eccentricity anſwer to the longitudes, but the latitudes do not indeed begin to approach each other but by ſuppoſing it to be very great, (four ſixteenths).

"Theſe reſearches ſeem to me to prove that we muſt ſuppoſe a very eccentric orbit; and I have begun to calculate a parabolic orbit as for a comet, and the elements I have found are as follows:

Inclination of the orbit	54°	58′	30″
Aſcending node	5^s	26	45 34
Place of the perihelion	3	23	$52\ 3\frac{1}{2}$
Perihelion diſtance	1·8432		

Time of the paſſage of the perihelion September 29, 1801, 16^h 48′.

The motion direct.

"Theſe

"These elements will correspond to three observations, of the 29th March, 7th and 16th April. I have omitted nothing that could render this calculation as exact as possible; but the heliocentric arc being only four degrees, we can expect only to attain nearly to exactness. The observation, therefore, of April 19, which Mechain was so kind as to communicate to me, gives an error in longitude of $-35''$, and in latitude of $-11''$. I am, however, far from wishing to exclude elliptic orbits, and I shall continue my researches with them in proportion as a greater number of observations are obtained; but, in the mean time, I was desirous to gratify the curiosity which the possibility of another new planet seems to have excited in the public.

Floreal 1, year 10. BURCKHARDT."

METEOROLOGY.

At Derby, an uncommon phænomenon appeared in the air a few mornings ago: it consisted of four circles, and part of another, in the following order:—The smallest, which was nearest to the sun, might be fifteen degrees diameter, with luminous spots to the right and left, like what are called *parahelia*; the second encompassed the former, and was twice as large; the third the size of the second, and joined its circumference to the west; and the fourth circumscribed all the others, and was touched upon the western side by part of another of the same diameter.

VACCINE INOCULATION.

This useful practice seems to be gaining ground in Spain. Dr. Francisco Piguillem has published the following work in recommendation of it:—*La Vacuna en Espaňa, o Cartas Familiares sobre esta nueva Inoculacion, escritas á una Señora*, por el Dr. Francisco Piguillem. That is, The Vaccine in Spain, or Familiar Letters on this new Inoculation, &c. Madrid, 8vo. In Catalonia, above 7000 persons were inoculated between the month of December 1800, and September 1801. "On the highest mountains and amidst the severest cold, in the deepest valleys and amidst the greatest heat, (says the letter from which this account was taken,) the inoculation has been attended with complete success."

A letter from Portugal to Dr. Alibert at Paris, after mentioning the opposition which has hitherto been made to the vaccine inoculation in that country, says,—"Some one has found here, in a Portuguese work, that the vaccine inoculation was practised at Lisbon and in the neighbourhood so

Zach are ſtill too few in number to admit of any deciſive opinion being formed reſpecting the nature of this intereſting body. If it be conſidered as a comet, its regular motion and whole appearance oppoſe any idea of this kind; but there is every probability in favour of its being a planet, though its orbit muſt have an uncommonly great inclination to that of our earth. Dr. Olbers ventures to conſider it as a new primary planet of our ſyſtem, moving between Mars and Ceres in an orbit very much inclined to the ſun. Its period of revolution he makes three years, its mean diſtance from the ſun $2\frac{1}{10}$th of the diſtance of our earth from the ſun, and the place of the aſcending node 5^s 20°.

Mr. Walker, lecturer on aſtronomy, has obſerved this body, and deſcribes it as being of a pale red colour, very faint, and leſs brilliant than the Ceres, viſible with a definable diſc with a magnifying power of 100 times.

As the diſtance of this ſmall body from the earth is increaſing, and its light decreaſing, aſtronomers muſt haſten to make good obſervations if they are deſirous of determining its orbit in an accurate manner: it will otherwiſe be difficult to find it again.

The following notice from the aſtronomer Burckhardt contains ſome further uſeful information on this intereſting diſcovery:

"The ſtar diſcovered by Dr. Olbers on the 28th of March has ſo great a reſemblance to a planet, that it was natural to ſuppoſe its orbit to be ſomewhat eccentric. I have placed it ſucceſſively between the earth and Mars, between Mars and Piazzi's planet, and between the latter and Jupiter. I have employed the eccentricities of one and of two tenths (thoſe of Mars and Mercury), but neither of theſe ſuppoſitions has ſucceeded. It is eaſy to make a very ſmall eccentricity anſwer to the longitudes, but the latitudes do not indeed begin to approach each other but by ſuppoſing it to be very great, (four ſixteenths).

"Theſe reſearches ſeem to me to prove that we muſt ſuppoſe a very eccentric orbit; and I have begun to calculate a parabolic orbit as for a comet, and the elements I have found are as follows:

Inclination of the orbit	- -	54° 58′	30″
Aſcending node	- -	5^s 26 45	34
Place of the perihelion	-	3 23 52	$3\frac{1}{2}$
Perihelion diſtance	- -	1·8432	

Time of the paſſage of the perihelion September 29, 1801, 16^h 48′.

The motion direct.

"Theſe

"These elements will correspond to three observations, of the 29th March, 7th and 16th April. I have omitted nothing that could render this calculation as exact as possible; but the heliocentric arc being only four degrees, we can expect only to attain nearly to exactness. The observation, therefore, of April 19, which Mechain was so kind as to communicate to me, gives an error in longitude of $-35''$, and in latitude of $-11''$. I am, however, far from wishing to exclude elliptic orbits, and I shall continue my researches with them in proportion as a greater number of observations are obtained; but, in the mean time, I was desirous to gratify the curiosity which the possibility of another new planet seems to have excited in the public.

Floreal 1, year 10. BURCKHARDT."

METEOROLOGY.

At Derby, an uncommon phænomenon appeared in the air a few mornings ago: it consisted of four circles, and part of another, in the following order:—The smallest, which was nearest to the sun, might be fifteen degrees diameter, with luminous spots to the right and left, like what are called *parahelia*; the second encompassed the former, and was twice as large; the third the size of the second, and joined its circumference to the west; and the fourth circumscribed all the others, and was touched upon the western side by part of another of the same diameter.

VACCINE INOCULATION.

This useful practice seems to be gaining ground in Spain. Dr. Francisco Piguillem has published the following work in recommendation of it:—*La Vacuna en España, o Cartas Familiares sobre esta nueva Inoculacion, escritas á una Señora*, por el Dr. Francisco Piguillem. That is, The Vaccine in Spain, or Familiar Letters on this new Inoculation, &c. Madrid, 8vo. In Catalonia, above 7000 persons were inoculated between the month of December 1800, and September 1801. "On the highest mountains and amidst the severest cold, in the deepest valleys and amidst the greatest heat, (says the letter from which this account was taken,) the inoculation has been attended with complete success."

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END OF THE TWELFTH VOLUME.

☞ The Binder is deſired to place Dr. PULTENEY's Head at the Beginning of the Volume, and the other Plates at the End.

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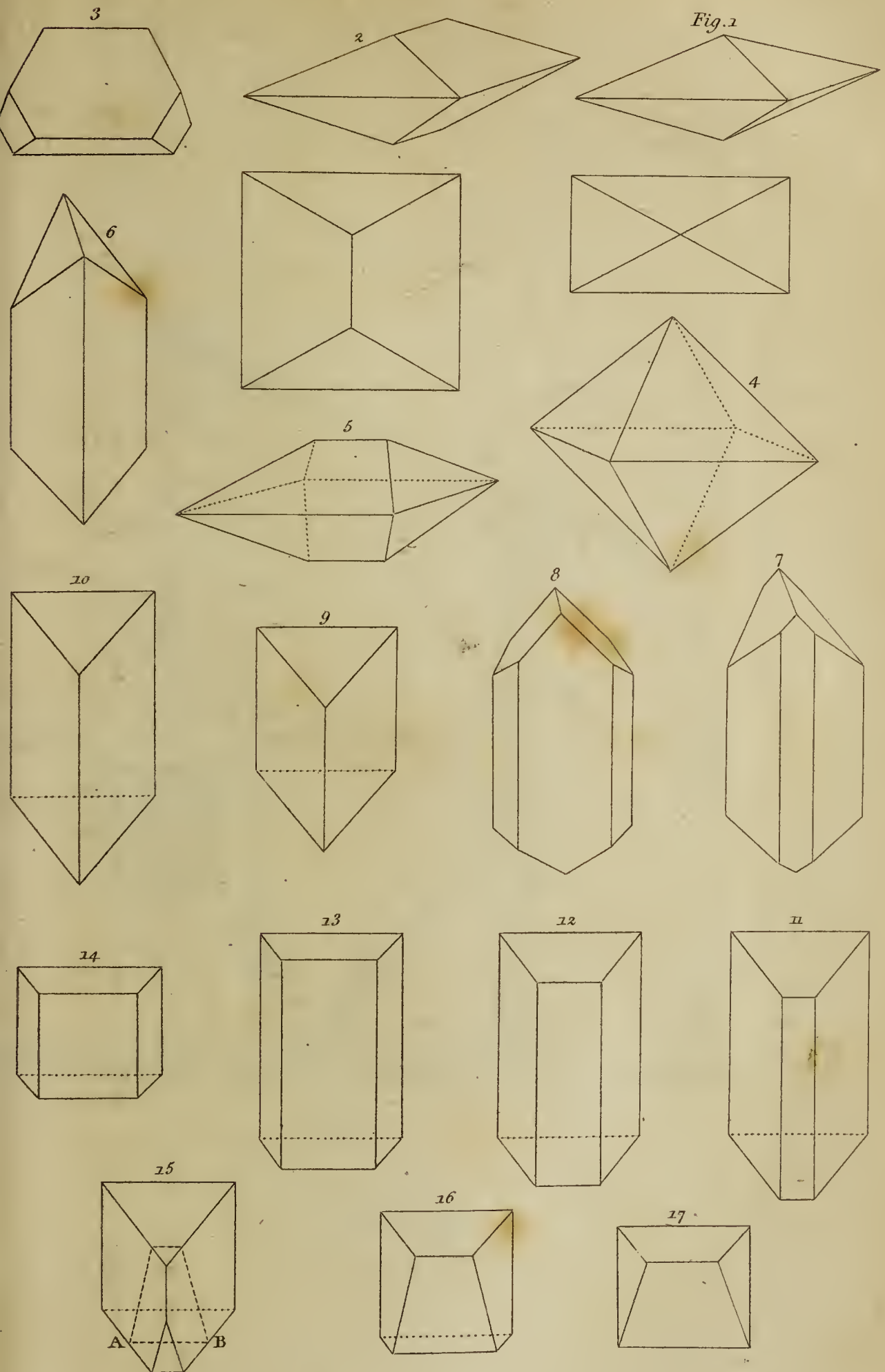

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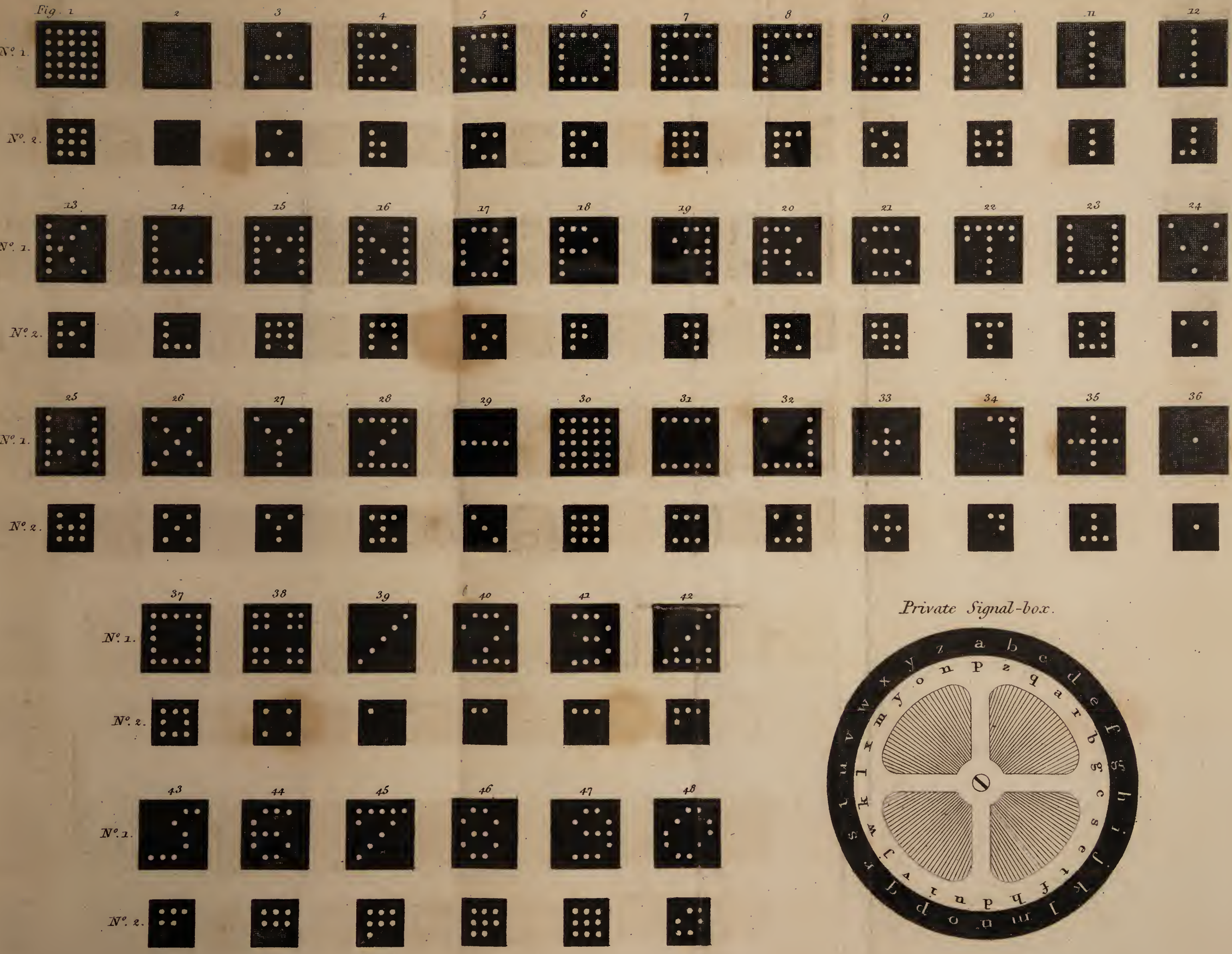

Private Signal-box.

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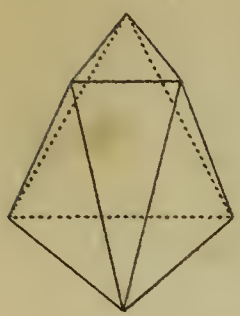

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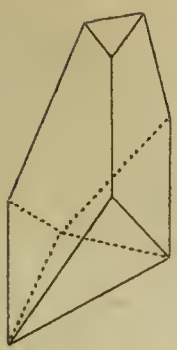

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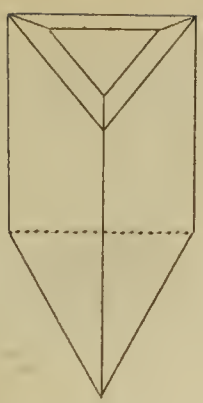

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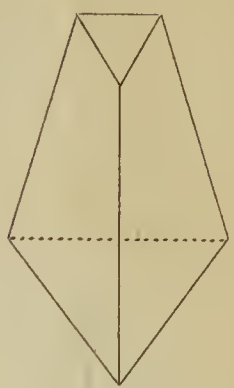

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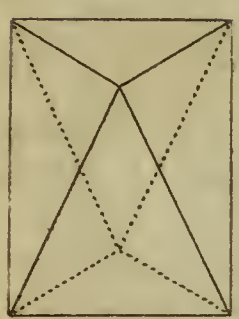

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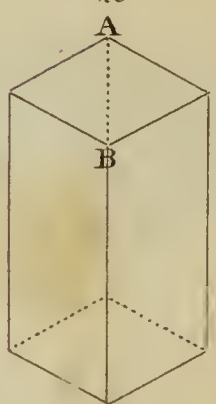

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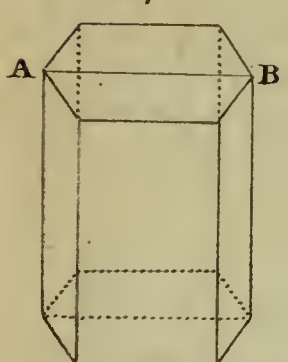

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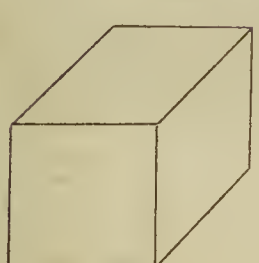

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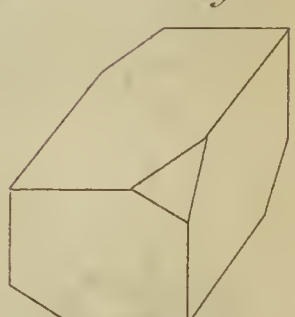

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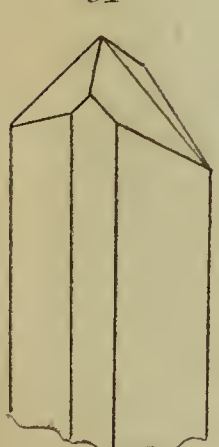

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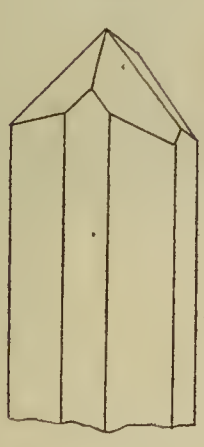

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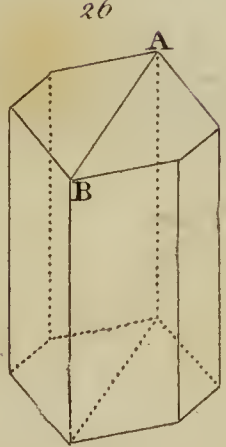

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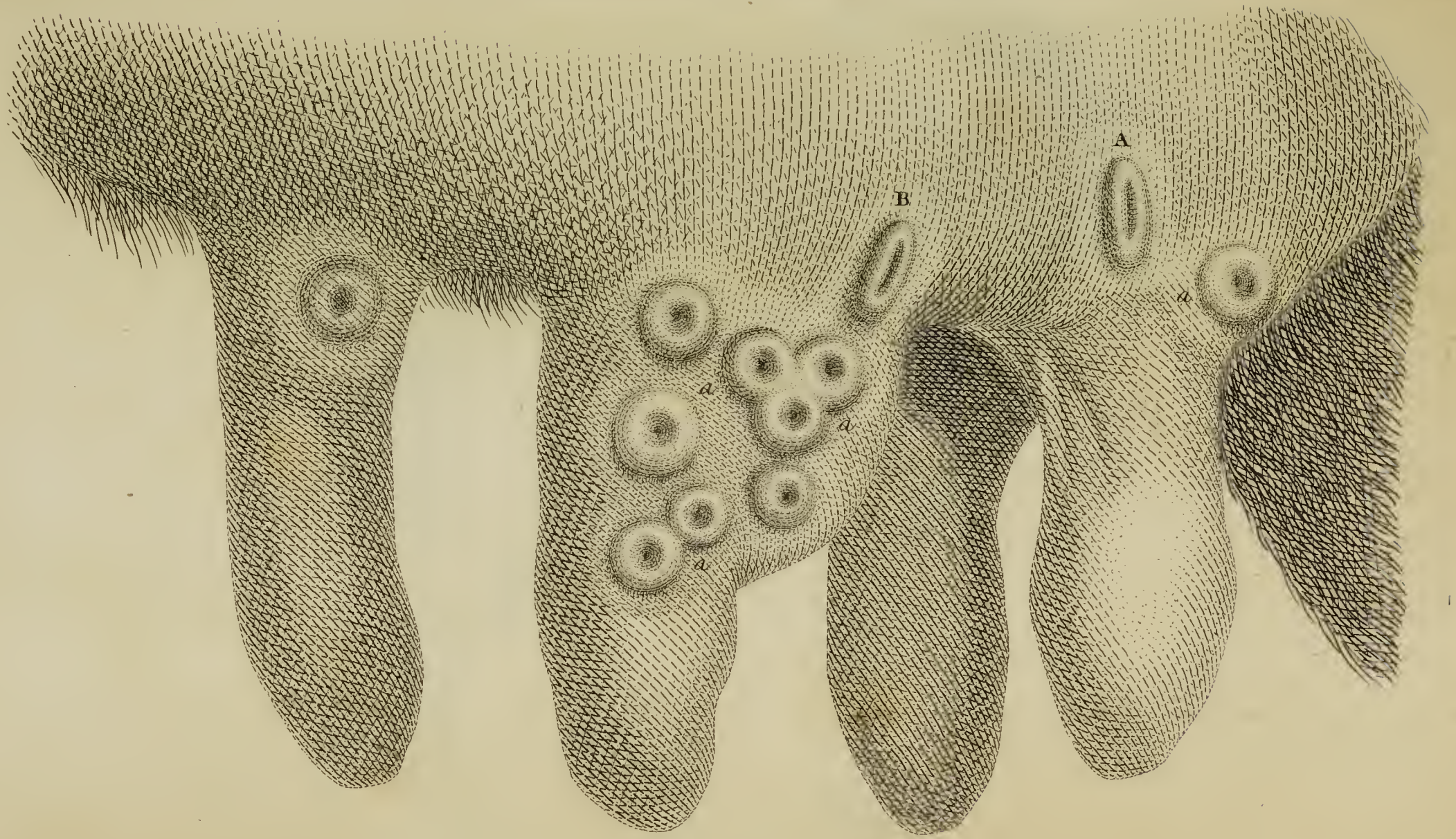

Lowry sculp

The Ibex, or Alpine Wild Goat.

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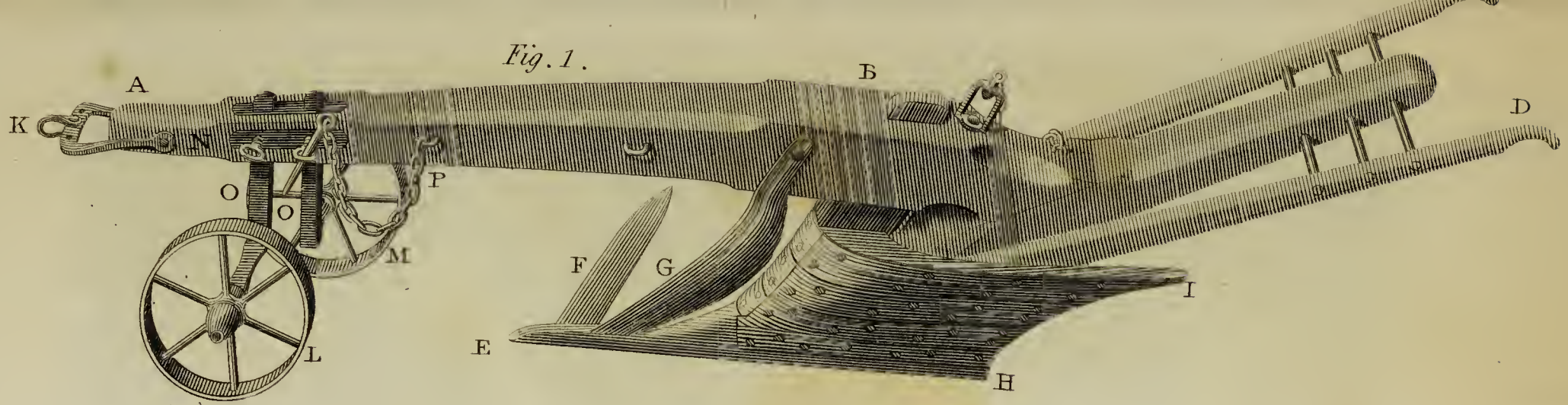
Fig. 1.



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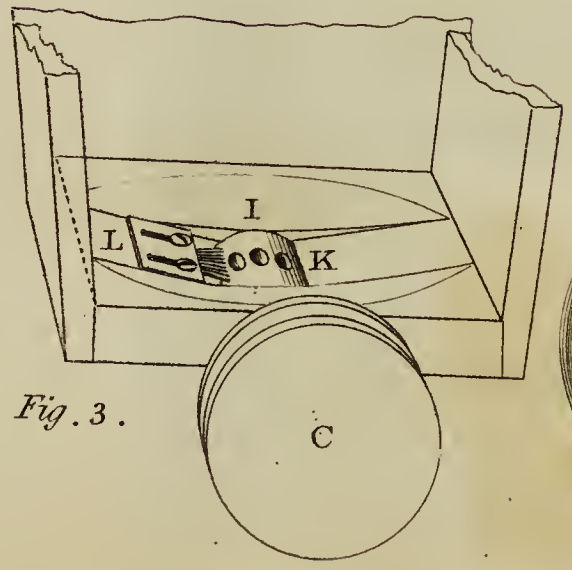

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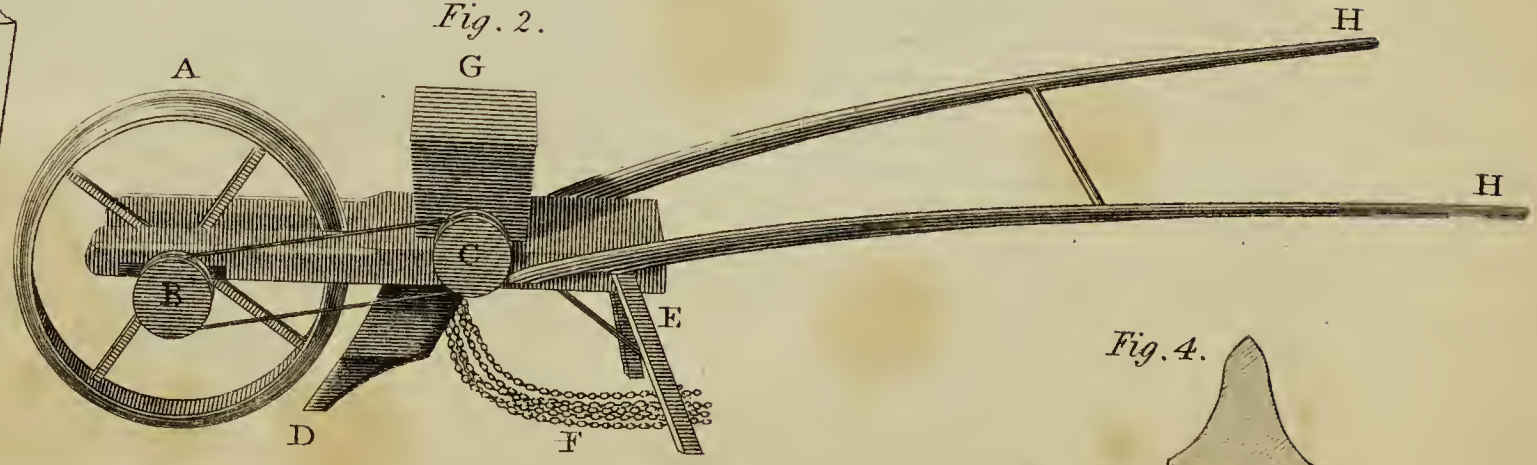
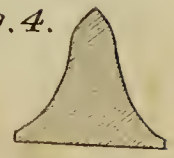
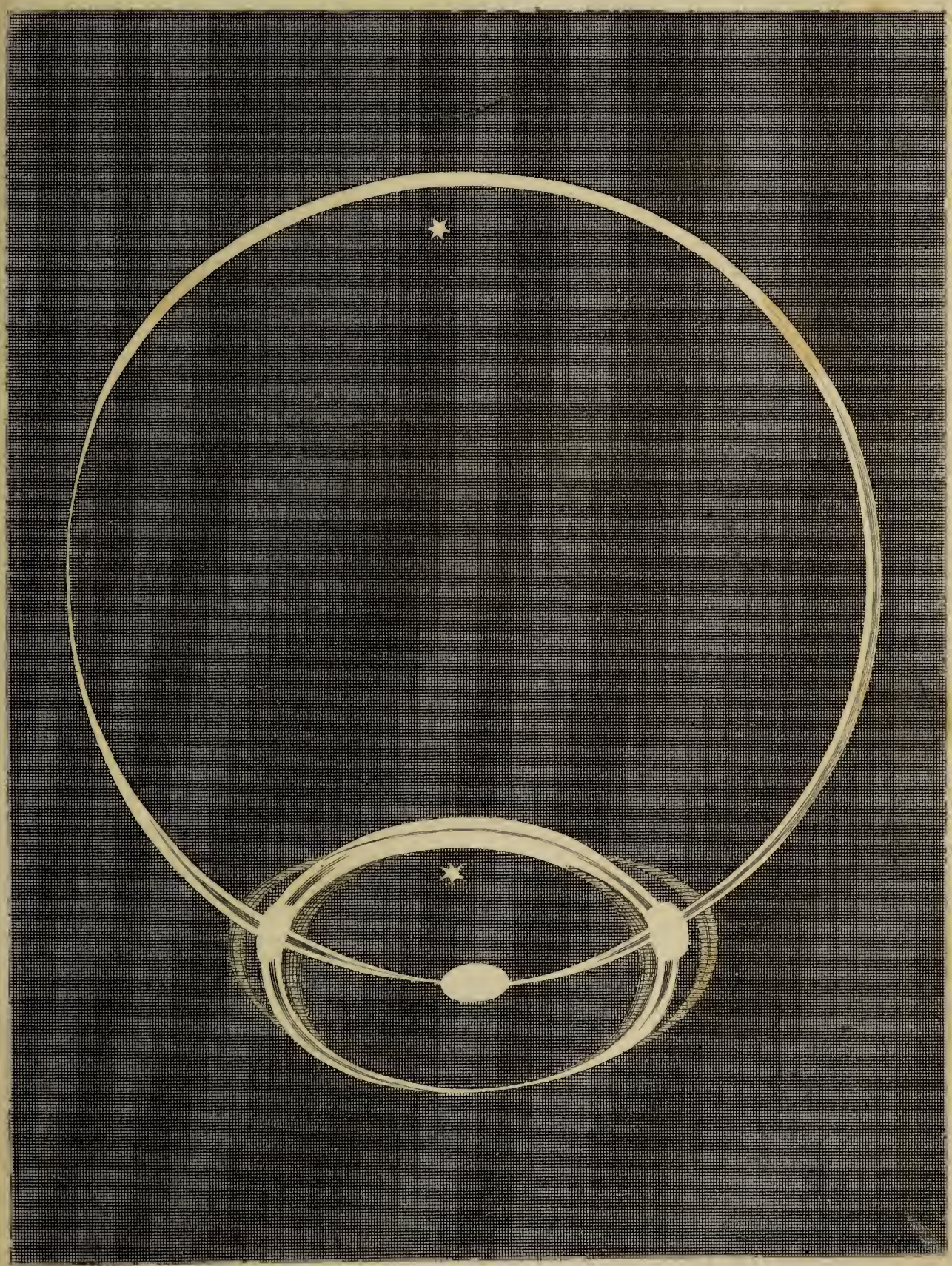


Fig. 4.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